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Lane, B. H., 1935. Suggestions to authors of papers submitted for publication by the United States Geological Survey: U. S. Geol. Survey, Washington, D. C. (For sale by Superintendent of Documents, Washington, D. C., 25c.)

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American Journal of Science

JULY 1951

PEAT SAMPLES FOR RADIOCARBON ANALYSIS: PROBLEMS IN POLLEN STATISTICS[1]

EDWARD S. DEEVEY, JR., AND JOHN E. POTZGER

ABSTRACT. In order to calibrate the pollen chronology by radiocarbon determination of age on pollen-dated peat and lake mud, it was necessary to obtain comparatively gigantic samples in sediments accessible only by boring. The proper solution is to use a sampler large enough to take five or ten pounds of wet sediment in one operation, and such a sampler has been devised. Its great size has proved a barrier to its operation at depths greater than about 6.5 meters, though satisfactory samples have been obtained at smaller depths. Before this instrument was contemplated, however, material was obtained by the method of multiple sampling, and the use of this method raises the difficult problem of the stratigraphic equivalence of samples.

The statistical foundation of pollen analysis has been little explored, and while the broad outlines of the pollen chronology are valid regardless of statistical considerations, small differences in pollen frequencies between successive samples have no statistical validity when the total numbers of grains counted are as small (100-200) as they usually are. In making comparisons between samples supposedly taken at the same level, significant differences in pollen frequencies may appear that have no meaning in the usual pollen-analytical sense, for they are based on short-term fluctuations in pollen sedimentation. Moreover, the errors of estimate of pollen percentages, based on the binomial frequency function, are large, and even in samples that are completely homogeneous with respect to their own binomial variance it may require the counting of very large numbers to obtain a precise estimate of the true frequency of a pollen type in the "universe" from which the sample was drawn. The labor involved is hardly justified if the existence of real short-term stratigraphic variation makes one uncertain that the universe sampled at any level is the correct one. It is also evident that long-term stratigraphic differences exist between the top and the bottom of any sample core 15 cm. or so in length, and while

[1] This investigation has been supported by Grants-in-Aid from the Connecticut Geological and Natural History Survey, the American Philosophical Society, and the Research Fund of the Scientific Research Society of America. The authors are profoundly grateful to these agencies for their generosity and for the dispatch with which the aid was granted, making possible field work in the season of 1949. Invaluable advice on statistical problems was obtained from Doctor John W. Tukey of Princeton University.

such a difference in age is assumed to be within the error of the age determination by radiocarbon content, the stratigraphic differences in pollen content may be great enough to give a false impression of non-equivalence between samples.

A sampling design, adequate to permit the recognition of equivalence among 6 samples and the rejection of 7 others, has been applied to a suite of samples from Lake Cicott Bog, Indiana. It involved (1) the preparation of 10 subsamples from each sample, so distributed as to include most of the long-term stratigraphic variance; (2) the counting of 100 pollen grains in each subsample, for a total of 18,000 grains; (3) analysis of variance, which showed that for the 6 samples uniformly high in pine pollen the variance between samples was smaller than that within the samples.

The remaining samples were studied by less complete sampling methods, but chi-square tests of homogeneity, *i.e.*, of departures from normally expected binomial variance, were passed satisfactorily by 3 suites of 31-32 samples from the center of Upper Linsley Pond, Connecticut, single subsamples having been taken from each sample. At a station on the bog mat, near the shore of the same lake, marked heterogeneity was encountered, resulting from gross disturbance of the stratigraphy by sinking pieces of wood. The disturbance is not shown by the standard pollen diagram for this station, a fact that emphasizes the statistical weakness of routine pollen work. At another locality, Johnson Camp, Minnesota, a suite of 12 samples, from which single rather large subsamples were drawn at random, proves to have been somewhat heterogeneous, though all samples certainly came from the pine zone. The advantages of the Kendall sum for quick comparisons of the variance between such samples are illustrated in connection with this series.

Some data have been collected bearing on the more usual problem in pollen stratigraphy, that of determining the significance of apparent differences between successive samples in a vertical profile. In a zone of rather great uniformity, the lower part of *C-1*, it is possible to demonstrate significant differences between the top and the bottom of a single peat borer sample (representing about 10 cm. depth). The statistical analysis of 25 replicates in each group confirms what the non-significant pollen diagrams suggest, that pine pollen was decreasing and beech pollen increasing in this zone. Replicate samples should be taken within 1 cm. of each other, or else large slices of the entire core should be homogenized before sampling, if such small but real long-term changes are to be correctly tested for significance. This recommendation amounts to a support of the sampling program advocated by Faegri and Ottestad, except that those authors believed that 2 samples within 1 cm. of each other should be sufficient, while we suggest 10. Following this program, the proper statistical procedure for testing significance is probably analysis of variance. As some of the assumptions underlying this procedure may not be adequately met, the Kendall sum should also be tried.

INTRODUCTION

THE chronology provided by stratigraphic sequences of pollen assemblages in fresh-water and marine deposits has been employed in a wide variety of dating problems, and the technique of "pollen analysis" has many practitioners. The

counting of pollen grains leads at once to statistical questions, yet it is possible to read widely in the subject without encountering any noteworthy appreciation of statistical methods on the part of authors, even those who use the term "pollen statistics" in their titles. Since the statistical foundations of pollen counting have never been adequately explored, it is not surprising that the literature abounds in conclusions drawn from data having no objective significance. Fortunately, the matter of statistical validity is relatively unimportant in ordinary stratigraphic practice, because postglacial pollen sequences reflect major shifts in the composition of the pollen flora, and as the chronology is built upon the recurrence of such shifts in many sections separated by great distances, the inference is correctly drawn that the shifts were real and resulted from undeniable changes of climate and vegetation.

The problem of obtaining large samples of pollen-dated peat and lake sediments for radiocarbon analysis exposed the statistical inadequacy of pollen research in an especially disheartening way. The discovery that natural radiocarbon (C^{14}) occurs in the biosphere in steady-state concentration equivalent to about 12.5 counts per minute per gram of carbon (Libby, Anderson, and Arnold, 1949; Arnold and Libby, 1949), and the realization that the age of fossil carbon can therefore be calculated with some exactness on the basis of its activity and of the known half-life of the element offered for the first time the possibility of direct calibration of the pollen chronology in terms of absolute age. Results of the dating program, undertaken on an international scale with the cooperation of Doctor Libby and the support of the Viking Fund, Inc., will be presented elsewhere. In this article we shall discuss the methods of collecting material, homogeneous with respect to age, in the quantities required for application of the new means of dating. Our task was more difficult than that of the archaeologists, many of whom were easily able to supply the requisite amounts of wood, textiles, or charcoal. In some European peat bogs that have been drained and excavated it has been a comparatively simple matter to remove a pound or so of material from a single stratigraphic horizon, but when the sediments are accessible only by boring it is expedient to resort to multiple sampling with the peat borer. This raises at once the troublesome question to which we now address ourselves:

when several different borer samples are obtained from the same level in the sediments, to what extent are they contemporaneous?

PREVIOUS WORK IN POLLEN STATISTICS

The paper of Barkley (1934) has often been cited, particularly by American workers, as a study of the statistical problems of pollen counting, and as the result has been a complacent acceptance of the utility of counts of 150 pollen grains at each level, it is necessary to discuss this paper rather critically. To be precise, Barkley's calculations gave no grounds for complacency, since they do not lead to the conclusion that 150 grains is an adequate number. Instead, they tend to show that 150 grains is a wholly inadequate number, but that accurate results cannot be obtained without an impossibly large amount of labor. For the prevailing misinterpretation of his position Barkley was not responsible, but it is unfortunately true that his methods were ill-adapted to the problem he set himself.

Barkley's method was to calculate correlation coefficients between two successive counts of 100 grains each, made on two parts of the same slide. The number of pollen types considered was about 30, including shrubs and herbs as well as forest trees. The results showed (1) that the correlation coefficient between two halves of a count of 200 is seldom as high as 0.9; (2) that it cannot be raised to 0.95 without making N several times as large; (3) that counts from "corresponding layers" in adjacent borings in the same deposit are so poorly correlated that it is senseless to try to improve their accuracy by counting larger numbers.

We believe, however, that a correlation coefficient is not a fair measure of the kind of reliability pollen workers seek. By including rare as well as common types in his analysis, Barkley gave undue weight to species whose probability of occurrence was less than 1/100; for such rare types alone the expected correlation coefficient would be zero, since a given type, observed in one count of 100, would not be found in the next count of 100. On the other hand, the accumulated probabilities of occurrence of several types, whose frequencies are all low though different among themselves, will affect the probability of occurrence of the four or five common types in a

way that would be exceedingly difficult to specify. Yet to confine attention to the common types alone would lead to the calculation of a correlation coefficient from five or six pairs of observations, and while its numerical value might be high its significance would be negligible.

The fundamental fallacy of applying correlation technique to pollen counts lies in the fact that pollen frequencies are not independent variates that can take any value, but estimates of probability of occurrence, each of which is affected by the true frequency of all the other types present in the sample. Barkley's study leaves untouched the question of the variance to be expected when frequencies are estimated by observed percentages and cast into stratigraphic diagrams.

This question has been attacked by several workers. The problem is essentially that of deciding, when m grains of a particular type have been counted out of a total number N, to what extent the observed frequency, calculated as m/N, or (in percentage) $100m/N$, agrees with the expected frequency p in a universe of pollen grains. If there is an expected frequency p the probability that the type will occur m times in a series of N independent trials is given by the binomial frequency function; in the absence of any particular hypothesis about p, such as would be given by a Mendelian ratio, for example, it is necessary to take m/N as the only available estimate of p, and, where percentages or proportions are used, the variance σ^2 of these proportions is given by $p(1-p)/N$. The standard deviation $\sigma = \sqrt{p(1-p)/N}$ decreases as N increases. On the assumption that binomial variance alone is involved in pollen counting, it is easy enough to calculate σ for any series of values of m and N, and by taking confidence limits equal to $\pm 2\sigma$ or $\pm 3\sigma$, the expected range of variation around the observed percentage can be looked up in a table (Faegri, 1945). Likewise, if a stratigraphic pollen diagram is constructed on the basis of counts of constant N, such as 150, a nomogram can also be constructed showing confidence limits for that value of N, and the probability that two percentages are significantly different can be estimated graphically (Westenberg, 1947). The decision, of course, is based on the fact that confidence limits for two different percentages, if drawn at $\pm 2\sigma$, will not overlap when the probability that they are really different is 95%; it is a graphical

means of saying that two percentages are significantly different if their difference exceeds twice its own standard error σ_D where $\sigma_D = \sqrt{\sigma_1^2 + \sigma_1^2}$.

Unfortunately the validity of this approach depends entirely on the unproved assumption that the variance of the percentages is in fact binomial. If additional sources of error are present the calculated binomial variance will underestimate the true variance; moreover the reliability of the usual methods of testing significance of differences comes sharply into question, owing to disturbances of the levels of probability obtained from tables of t or F. It is not difficult to conceive of other sources of error in pollen counting: (1) There is the sampling error incident on the preparation of a pollen slide from a smear of sediment in KOH. (2) An additional sampling error is introduced by the necessity of taking a small amount of material from a larger core as it comes from the peat borer. (3) If the distribution of pollen grains under the cover slip is not random, *e.g.* if larger grains are found nearer the margins of the slide, a further disturbance is introduced. Wenner (1948) has found that this kind of disturbance is normally present, and has shown further that this error explains differences in successive counts of 100 grains made on a single slide by Bowman (1931). (4) Differences in method of preparation are certain to introduce errors, if they affect the frequencies of various pollen types in differential fashion, or if they lead to different forms of non-random distribution of pollen types under a cover slip. This problem has also been discussed by Wenner, whose data showed that the KOH method and the Erdtman acetolysis method do not always produce the same results. Finally, (5), the most serious additional source of "error" is *stratigraphic*. This sort of error can be expected to be negligible if a small bit of sediment is removed from a core and examined in a KOH smear; it becomes larger as the thickness of the subsample increases, and it can be expected to contribute the largest share of the variance if the subsample, from which the pollen slide is made, includes material from a layer, perhaps a centimeter or more thick, that has required an appreciable span of years for its deposition.

By using a sampling program that minimizes stratigraphic

error, Faegri and Ottestad (1949) have found that the variance of pollen counting is in fact close enough to the binomial variance so that serious discrepancies are not to be expected in ordinary pollen work. The proof is not easy to follow, and the only reference made is to Mather (1946), a work that most ecologists will find difficult. An elementary statement of the method is given by Snedecor (1946); it consists of the chi-square test of homogeneity. When a large sample of peat is made homogeneous with respect to stratigraphic differences by boiling it in KOH, and subsamples are then drawn off, prepared as pollen slides, and counted, binomial variance can be calculated for each major pollen type present in each subsample. One can make no hypothesis about the true frequency p of any pollen type except that it is most accurately estimated by the mean frequency of that type, $M_{tot}/N_{tot} = \bar{p}$. The observed variance around the mean frequency can only be compared with the variance calculated on the hypothesis of binomial distribution; this leads to a table of terms of the form $\frac{(p-\bar{p})^2}{\bar{p}}$ where p is the observed pollen percentage in each subsample and $\bar{p}$ is the mean of subsample values of p. The table has R rows (corresponding to the number of subsamples) and C columns, (corresponding to the different pollen types tested); Faegri and Ottestad used four columns for birch, oak, alder, and hazel, and a fifth column for all the rest of the species present. The table sum of terms of this form is equal to χ^2, with $(R-1)(C-1)$ degrees of freedom; for five columns and eight subsample rows there are 28 degrees of freedom. When the number of degrees of freedom is so large a simple calculation, the basis of which is given by Snedecor, indicates that χ^2 will not show a significant departure from homogeneity with respect to the assumed binomial variance unless its value materially exceeds the number of degrees of freedom. Four large samples, divided into 7, 8, or 9 subsamples, and tested in this way, gave no significant departure from homogeneity, and Faegri and Ottestad concluded that essentially all the variance was binomial. Moreover, 50 counts of 10 grains each, distributed over a vertical depth of 1 meter in a "layer of synchronal appearance" were also tested, and again the observed variance did not materially exceed the binomial variance; the observed

variance was greater for all pollen types, as would be expected from the fact that a small amount of stratigraphic error was deliberately included, but the departure was non-significant.

Faegri and Ottestad reported that their examination of similar data published by Woodhead and Hodgson (1935) gave "no indication of significant deviations from the hypothesis of binomial distribution." We have also tested the twenty counts of 100 grains each reported for a single slide by Bowman (1931) with the same result. It appears, therefore, that the hypothesis of binomial variance is sufficiently correct for purposes of significance tests in ordinary work, where stratigraphic variance is avoided for any one slide and emphasized for successive slides. It must be reiterated, however, that the empirical data supporting this conclusion are meager, and that the assumption of binomial variance is likely to prove fallacious under easily imaginable circumstances.

THE PROBLEM OF STRATIGRAPHIC EQUIVALENCE

When several independent samples are taken with a peat borer from the same level in a bog, the most careful field work gives no automatic assurance that they are stratigraphically equivalent, and in fact it is plain that if the samples are spaced more than a few feet apart in a small bog, stratigraphic differences are certain to appear. Moreover, a sample with a length of some 15 cm. will certainly show stratigraphic differences between top and bottom. The accuracy of the radiocarbon method of dating is not so great that a stratigraphic error equivalent to 15 cm. in a postglacial section 10 or 15 m. thick should be cause for alarm; but it is easy to imagine (and it will be demonstrated below) that differences in pollen flora could exist between the top and the bottom of a single borer sample. Whether or not such differences are climatically significant, or whether or not they imply serious discrepancies of absolute age, they may be expected to influence comparisons between samples when the only criterion of difference is found in the pollen flora.

The excruciating quality of this problem was only dimly perceived when we began our efforts to obtain pollen-dated peat samples for radiocarbon determination. Most of our

sampling design reflects this early bewilderment, which we shall attempt to spare the reader by presenting first the results of the latest and most illuminating analysis.

A suite of 13 samples was taken through the ice on Lake Cicott Bog, Cass County, Indiana, by Potzger. The aim was to obtain a bulk sample at the top of the pine pollen zone (Zone *B* in the terminology of Deevey, 1943, 1948, 1949) which previous studies by Smith (1937) had shown to lie at 22 or 23 feet. After the first six samples had been taken at 22 feet with a Hiller borer having a diameter of 1¾ inches, a field check of the material suggested that higher pine pollen frequencies were to be found slightly lower down, so the remaining seven samples were taken at 23 feet. Further work was impossible owing to ice conditions. Ten small subsamples were then removed from each sample, five from each face of the more or less rectangular chunks, an effort being made to take samples from near the top and from near the bottom of each chunk, as well as from the intermediate parts. Separate slides were prepared for each of the 130 subsamples, and 100 forest tree grains were counted in each.

The results are given *in toto* in table 1, where it may be seen that while the first six samples (A through F) are not consistently different from the last seven (G through M), there is a clear division into six samples (B,G,H,I,L,M) showing consistently high percentages of pine pollen, four samples (A,C,D,K) with consistently low pine percentages, and three (E,F,J) that are intermediate. These results, though obvious on inspection, required statistical confirmation, before the six high-pine samples could safely be fused for radiocarbon analysis.

It was first necessary to decide whether the percentages for any one or all types have a distribution within any one sample that is sufficiently close to normal so that the usual tests of significance can be applied. This would not be expected to be the case with ordinary percentages subject to binomial variance, but if the variance is binomial the appropriate procedure is to transform the percentages to angles by the formula

$$\text{angle} = \text{arc sin } \sqrt{\text{percentage}},$$

or, more simply, by looking up the angular values in the table given by Snedecor (1946, table 16.8). A simple test

TABLE 1

Results of counting 100 forest tree pollen grains in 10 subsamples from each of 13 Hiller borer samples from Lake Cicott Bog, Indiana, 22 and 23 feet.

Sample Number		Pine	Oak	Birch	Elm	Hickory	Maple	Hornbeam	Hemlock	Spruce	Beech	Walnut	Basswood	Fir	Ash	Sweet Gum
G	1	40	30	6	12	6	1	3	0	1	1	0	0	0	0	0
	2	42	30	10	11	1	2	1	0	0	1	1	1	0	0	0
	3	30	31	11	18	1	2	1	0	0	0	3	1	2	0	0
	4	40	29	9	15	1	3	0	0	0	0	2	1	0	0	0
	5	33	29	10	8	3	1	0	0	1	1	0	1	3	0	0
	6	27	44	12	8	1	2	1	0	1	0	4	0	0	0	0
	7	23	46	4	16	5	0	1	0	0	1	2	0	1	1	0
	8	33	30	11	18	3	1	0	0	1	0	1	2	0	0	0
	9	31	42	5	16	4	1	0	0	0	0	0	1	0	0	0
	10	26	45	9	12	1	2	1	0	0	0	3	1	0	0	0
H	1	27	49	2	14	5	0	0	0	0	0	2	0	1	0	0
	2	30	27	12	26	1	0	0	1	0	0	3	0	0	0	0
	3	25	43	10	9	7	2	2	0	1	0	1	0	0	0	0
	4	39	33	9	12	3	1	1	0	0	1	1	0	0	0	0
	5	32	39	7	7	5	6	0	0	1	0	1	1	0	1	0
	6	36	40	3	18	0	0	0	0	0	0	2	0	0	1	0
	7	28	40	8	14	4	1	1	0	0	0	3	0	1	0	0
	8	19	45	13	15	2	1	1	0	2	0	2	0	0	0	0
	9	24	46	10	10	4	0	0	0	1	0	5	0	0	0	0
	10	24	40	14	13	4	2	0	0	0	1	2	0	0	0	0
I	1	43	30	6	14	2	0	1	1	1	0	2	0	0	0	0
	2	27	43	12	10	2	1	0	1	0	0	3	1	0	0	0
	3	9	65	1	14	7	1	0	0	0	1	2	0	0	0	0
	4	39	39	5	9	3	0	0	0	1	1	2	0	1	0	0
	5	27	32	15	15	1	1	1	0	3	1	2	2	0	0	0
	6	46	23	10	13	2	1	1	0	0	0	4	0	0	0	0
	7	49	26	12	8	2	1	0	0	0	1	1	0	0	0	0
	8	34	37	12	10	1	1	0	0	1	0	2	1	1	0	0
	9	5	57	4	18	11	1	1	0	0	1	1	1	0	0	0
	10	30	44	12	9	1	1	1	0	0	1	0	0	1	0	0
L	1	39	30	11	12	2	0	1	0	1	0	4	0	0	0	0
	2	28	41	8	19	1	1	0	0	0	1	1	0	0	0	0
	3	23	29	6	35	2	2	1	0	1	0	1	0	0	0	0
	4	28	39	5	15	6	2	1	1	0	0	3	0	0	0	0
	5	9	57	7	22	3	0	1	0	0	1	0	0	0	0	0
	6	31	40	9	11	1	0	1	0	0	0	5	1	0	1	0
	7	37	38	9	9	1	1	0	0	0	0	2	2	1	0	0
	8	31	50	7	6	3	1	1	0	0	0	1	0	0	0	0
	9	33	32	9	18	5	1	0	0	0	0	2	0	0	0	0
	10	17	37	16	14	4	1	2	0	0	1	8	0	0	0	0
M	1	40	37	9	8	0	1	0	1	0	0	3	0	1	0	0
	2	7	62	6	13	8	0	0	0	0	0	1	1	2	0	0
	3	31	39	7	16	1	2	0	0	0	0	1	1	2	0	0
	4	34	44	6	11	0	2	0	0	0	0	1	1	1	0	0
	5	30	21	23	18	1	0	0	0	3	0	1	0	3	0	0

Sample Number		Pine	Oak	Birch	Elm	Hickory	Maple	Hornbeam	Hemlock	Spruce	Beech	Walnut	Basswood	Fir	Ash	Sweet Gum
	6	27	39	14	14	3	0	0	0	0	0	1	1	1	0	0
	7	41	29	9	15	1	0	0	0	1	0	1	0	3	0	0
	8	27	44	8	16	1	2	1	0	0	0	1	0	0	0	0
	9	31	39	6	12	6	0	1	0	1	0	3	0	1	0	0
	10	47	27	8	13	2	0	0	0	1	0	2	0	0	0	0
B	1	24	41	13	13	0	2	0	0	1	1	2	1	2	0	0
	2	22	49	4	15	4	2	0	0	0	1	2	1	0	0	0
	3	28	42	12	13	0	1	1	0	1	0	1	1	0	0	0
	4	22	39	10	18	3	1	1	0	1	1	3	0	0	1	0
	5	27	37	12	18	3	0	0	0	0	1	2	0	0	0	0
	6	35	35	8	15	4	1	0	0	1	1	0	0	0	0	0
	7	16	46	13	17	2	1	0	0	2	2	1	0	0	0	0
	8	43	34	6	14	2	0	0	0	0	0	1	0	0	0	0
	9	29	40	4	15	2	3	1	0	1	1	3	0	1	0	0
	10	33	38	2	15	5	2	0	0	0	0	3	1	1	0	0
A	1	0	76	5	8	7	0	0	0	0	1	2	0	0	1	0
	2	0	65	2	15	9	3	1	0	1	2	0	2	0	0	0
	3	3	62	5	13	7	3	1	0	0	3	3	0	0	0	0
	4	2	71	1	13	9	1	0	0	0	1	0	1	0	1	0
	5	3	64	8	17	3	1	1	0	0	1	1	1	0	0	0
	6	5	72	1	12	7	0	1	0	0	0	2	0	0	0	0
	7	0	75	4	11	4	1	0	0	0	0	3	1	0	1	0
	8	6	65	1	13	7	4	2	0	0	0	1	0	0	0	1
	9	2	60	5	22	8	0	0	0	0	0	1	2	0	0	0
	10	0	63	7	15	10	1	0	0	0	0	2	0	0	2	0
C	1	1	77	1	10	7	1	0	0	0	1	2	0	0	0	0
	2	1	70	5	12	6	1	0	0	0	3	1	1	0	0	0
	3	6	72	0	13	3	1	0	0	1	2	2	0	0	0	0
	4	3	77	1	11	4	2	0	0	0	1	1	0	0	0	0
	5	2	67	5	16	7	1	0	0	1	0	1	0	0	0	0
	6	0	67	0	14	11	1	0	0	0	2	2	2	0	0	1
	7	3	71	2	12	4	1	0	0	0	2	5	0	0	0	0
	8	2	71	4	11	4	1	0	0	0	4	1	2	0	0	0
	9	2	78	5	8	4	0	0	0	0	0	3	0	0	0	0
	10	1	73	3	14	4	1	1	0	0	0	2	1	0	0	0
D	1	4	72	2	13	3	1	0	0	1	1	2	0	0	0	1
	2	4	76	0	9	5	2	0	0	1	1	2	0	0	0	0
	3	4	68	4	20	4	1	0	0	0	2	2	0	0	0	0
	4	3	73	2	12	5	1	0	0	0	0	2	2	0	0	0
	5	3	69	5	12	10	0	0	0	0	1	0	0	0	0	0
	6	4	72	4	8	8	1	0	0	0	0	3	0	0	0	0
	7	1	75	6	11	5	0	0	0	0	1	1	0	0	0	0
	8	1	72	5	10	6	1	1	0	0	2	2	0	0	0	0
	9	3	70	1	9	10	1	0	0	0	3	3	0	0	0	0
	10	2	70	6	11	5	0	0	0	0	3	2	1	0	0	0
K	1	2	65	5	19	5	1	0	0	0	3	0	0	0	0	0
	2	3	58	6	19	9	1	0	0	0	2	1	1	0	0	0
	3	4	64	2	14	10	1	1	0	0	1	1	1	0	0	1
	4	0	70	8	12	8	1	0	0	0	0	1	0	0	0	0
	5	1	64	3	11	14	1	0	0	0	1	5	0	0	0	0
	6	3	66	8	11	11	0	0	0	0	1	0	0	0	0	0

	Sample Number	Pine	Oak	Birch	Elm	Hickory	Maple	Hornbeam	Hemlock	Spruce	Beech	Walnut	Basswood	Fir	Ash	Sweet Gum
	7	3	67	4	15	7	3	0	0	0	1	0	0	0	0	0
	8	3	61	5	18	10	0	0	0	0	1	1	1	0	0	0
	9	2	63	4	16	9	1	2	0	0	0	2	1	0	0	0
	10	1	66	5	14	12	1	0	0	0	0	1	0	0	0	0
E	1	18	42	6	25	7	1	0	0	0	0	1	0	0	0	0
	2	20	55	5	12	5	1	1	0	0	0	1	0	0	0	0
	3	6	55	8	18	10	2	0	0	0	0	1	0	0	0	0
	4	4	63	6	15	8	1	1	0	0	1	1	0	0	0	0
	5	18	46	10	14	5	1	1	0	1	1	3	0	0	0	0
	6	22	47	7	12	7	1	0	0	0	1	2	1	0	0	0
	7	8	59	6	16	4	1	0	0	1	2	3	0	0	0	0
	8	31	41	9	12	3	2	1	0	1	0	0	0	0	0	0
	9	8	66	3	10	4	3	1	0	0	1	3	1	0	0	0
	10	9	56	5	18	6	1	1	0	0	0	4	0	0	0	0
F	1	8	55	7	15	7	2	1	0	0	0	4	1	0	0	0
	2	1	65	8	18	5	2	0	0	0	0	0	1	0	0	0
	3	13	57	3	20	6	1	0	0	0	0	0	0	0	0	0
	4	11	51	7	19	8	1	2	0	0	0	1	0	0	0	0
	5	9	53	3	24	6	1	0	0	1	0	2	1	0	0	0
	6	10	64	5	9	9	1	0	0	0	0	2	0	0	0	0
	7	7	52	7	18	14	2	1	0	0	0	4	0	0	0	0
	8	10	53	4	20	4	3	1	0	0	0	4	1	0	0	0
	9	5	58	6	19	3	3	0	0	0	2	3	1	0	0	0
	10	5	40	12	23	8	4	1	0	0	1	5	1	0	0	0
J	1	25	45	10	11	3	0	0	1	0	0	1	0	0	0	0
	2	13	55	4	16	7	1	0	0	0	1	2	0	1	0	0
	3	3	68	1	12	8	1	0	0	0	1	5	1	0	0	0
	4	16	53	7	16	5	0	0	0	0	0	2	0	0	0	1
	5	12	57	5	15	10	0	0	0	0	1	0	0	0	0	0
	6	22	47	4	15	8	1	0	0	0	0	3	0	0	0	0
	7	34	38	6	15	1	1	0	0	1	0	2	0	0	0	0
	8	8	63	10	12	4	1	0	0	0	0	2	1	1	0	0
	9	19	46	8	19	2	1	1	0	0	0	4	0	0	0	0
	10	16	56	11	13	2	0	0	0	1	0	1	0	0	0	0

of normality among a series of variates is to plot them in order of rank against values of rankits given by Fisher and Yates (1938, table XX). When this was done for a number of subsample percentages it appeared that in most cases the distributions were not normal, but that little improvement was given by the angular transformation. Evidently the variances are neither binomial nor of any other simple sort, but in order to test the matter on a larger cluster of points than ten, the observed variances (σ^2) were computed by the ordinary method for all percentages (37 in number) in samples C, G, and J. When the observed variances were

plotted against their respective means, a scatter diagram resulted, indicating once more that whatever form the variance takes it is not normal. The same result was obtained when the percentages were all converted to angles. At this point the cumbersome angular transformation was dropped, since it is plain that extraneous variance makes so large a proportion of the total that the logical basis of the transformation is uncertain.

Our inability to specify the character of the variance observed in table 1 certainly arises from the fact that stratigraphic differences exist within samples, and these differences vary from sample to sample. Standard methods of testing significance have therefore to be regarded with great suspicion, since they have been devised for restricted conditions, and require assumptions that are not likely to be met even approximately in this case. The powerful technique of analysis of variance is fully satisfactory only when the data to be analyzed are subject to normally distributed errors. Eisenhart (1947) and Cochran (1943, 1947), among others, have discussed the seriousness of various kinds of failure to conform to the assumptions underlying the technique, without providing any recipes that can be regarded as infallible under the present highly peculiar circumstances. It can be said, however, that the algebra of the procedure is unobjectionable, in that it describes the variance inherent in the data, and that the chief difficulties introduced by non-normality arise in the interpretation of significance tests by the *F*-ratio and in the estimation of components of variance. Even these difficulties are not likely to be disastrous if a positively significant *F*-ratio has been obtained (private communication from J. W. Tukey).

As it happens, we are not in grave danger of being led to false conclusions about the Lake Cicott samples, since we are not interested in finding significant differences, but in choosing samples that display none. We shall be wise also not to attach undue weight to apparent differences in percentages where the percentages are small, but to focus attention on major components whose binomial errors depart least from normality. Estimates of the components of variance, however, must be taken with some reserve.

An example of the calculation is given in table 2. For the

sake of convenience we have followed step-by-step an analysis worked out by Cochran (1943) for percentages based on unequal numbers. The ratio of binomial to total variance is computed by the formula

$$\frac{\bar{f}\ (1-\bar{f})}{s^2\bar{n}_h}$$

TABLE 2

Analysis of variance of pine percentages, high-pine samples (B, G, H, I, L, M) only, from Lake Cicott Bog, Indiana.

	Degrees of Freedom	Sum of Squares	Mean Square	F-ratio
Total	59	46.13		
Samples	5	3.06	0.611	0.765
Individual Counts	54	43.07	0.799	

Mean pine frequency $= \bar{f} = 0.298$; $1 - \bar{f} = 0.702$

Binomial variance $= \dfrac{\bar{f}\ (1 - \bar{f})}{\bar{n}} = 0.002096$

Extraneous variance $= \dfrac{s^2 - \bar{f}\ (1 - \bar{f})}{n_h} = \dfrac{0.799 - 0.2096}{100} = 0.005894$

$\dfrac{\text{Binomial variance}}{\text{Total variance}} = 26.2$ per cent.

where $\bar{f}$ is the mean frequency of a pollen type, s^2 is the error mean square, and $\bar{n}_h$ is the harmonic mean of the numbers on which the percentages are based. As the numbers are all the same (100) in our case, the harmonic mean $\bar{n}$ and the arithmetic mean $\bar{n}$ on which the binomial variance is calculated are identical.

When this method is applied to all the major pollen types in the high-pine, low-pine, and intermediate-pine groups of samples, the results are as shown in table 3. A few apparently significant *F*-ratios emerge, none of them in the high-pine group in which we are mainly interested, where in all cases the variance between samples is less than the variance within individual counts. The estimates of the binomial component of variance are probably not to be taken seriously except in so far as they are uniformly low for the (most homogeneous) high-pine group.

Regarding the stratigraphic equivalence of the six high-

pine samples, then, we can say that there is no evidence against it, and accordingly they have been fused for purposes of radiocarbon dating.

The other seven samples are of no further interest in this connection. The fact remains that all 13 came from within one foot of each other in a vertical direction, and all were taken within an area of 10 by 15 feet on the surface of the bog. One would like very much to know whether such differences as those between high-pine and low-pine samples are real, and thus whether they reflect significant differences in vegetation and climate.

We have emphasized our belief that this sort of question cannot be answered decisively by comparing variances of so unorthodox a character, and in a later section we discuss the problem on the basis of better data. We have nevertheless attempted to see what can be said on the matter, without insisting on the validity of the results. One fact appears incontrovertible, and has not been subjected to statistical quibbling: differences between high-pine and low-pine samples in respect to pine and oak pollen are consistent, and strongly suggest that in the segment of time that corresponds to one foot of peat deposition, there was a significant shift from pine vegetation to oak vegetation. What about the minor components of the pollen flora?

For the comparatively common types, birch and hickory, we have thought it not without interest to try a few sample-by-sample comparisons by means of analysis of variance. Since the proportion of binomial to total variance is generally higher for birch and hickory than for the three major components, pine, oak, and elm, we have made the comparisons in terms of angular equivalents of the original percentages. For samples A and B, where the birch percentages are 3.9 and 8.4, respectively, the difference appears statistically significant, as it does for samples I and M, where the percentages are 5.0 and 9.6. Differences in hickory percentages tend to fall in the opposite direction, and samples A and L, where the difference (7.1 and 2.8 per cent, respectively) is greatest, seem to differ significantly. The difference between the highest high-pine value for hickory in sample H (3.5 per cent) and the lowest low-pine value (5.4 per cent) in sample C falls just above the 5% significance level. We

have lacked the patience to carry this sort of examination to its dubious conclusion, but it appears not improbable that the top of the pine zone differs from the bottom of the oak zone in having more birch and less hickory.

TABLE 3

Analysis of variance of major pollen types by the method of Cochran (1943)

	Pollen Type	Mean Frequency, %	*F*-ratio	Ratio, binomial: total variance, %
High-pine	Pine	29.8	0.765	26.2
Samples	Oak	39.0	0.234	28.8
	Birch	8.9	0.100	49.03
(B, G, H, I, L, M)	Elm	14.0	0.970	54.84
	Hickory	2.8	0.394	52.30
Low-pine	Pine	2.3	0.570	97.68
Samples	Oak	68.8	*7.31	100
	Birch	3.8	2.08	75.67
(A, C, D, K)	Elm	13.1	2.47	100
	Hickory	7.0	*5.59	92.98
Intermediate-	Pine	13.0	*3.84	20.50
pine Samples	Oak	53.7	0.146	37.23
	Birch	6.4	0.597	82.66
(E, F, J)	Elm	15.9	2.35	87.93
	Hickory	6.0	1.27	71.16

* *F*-ratio significant beyond 5% level of probability.

Stratigraphic variance, unlike binomial variance, can never be controlled, for obviously it will differ with local stratigraphic conditions. It can be studied and allowed for, however, whenever the type of problem warrants the enormous amount of work required. The results reported here are encouraging in one sense, since they show that multiple sampling with a peat borer can yield at least a few stratigraphically equivalent samples. They are discouraging, first, because they provide no means whereby the obtaining of equivalent samples can be ensured during the field work, and second, because of the labor involved in demonstrating equivalence where it exists.

USE OF THE KENDALL SUM TO TEST HOMOGENEITY OF SAMPLES

The method of analysis of variance used to decide the question of stratigraphic equivalence in the Lake Cicott samples is not only of debatable applicability, but cumber-

some; the counting program, involving 13,000 pollen grains, was arduous in the extreme; and, had the homogeneity of the six high-pine samples not been fairly obvious on inspection, the results might not have been so decisive. A relatively new and unfamiliar procedure has been employed to examine the homogeneity of a composite sample from a bog near Johnson Camp, Minnesota. It involves a statistic called the Kendall sum (Kendall, 1938; Wilcoxon, 1945, 1947, 1950). In computing the Kendall and Wilcoxon sums, we have used the unpublished lecture notes by J. W. Tukey, which are available at Yale University.

The twelve samples obtained from a level of 8 feet in this bog were believed, on the basis of counts of 100 grains made on random subsamples, to be sufficiently homogeneous for the purpose, and they were fused and sent to Doctor Libby. As the age reported seems rather old for a pine zone sample, it is probable that if there is any lack of homogeneity it is not the result of contamination by younger material. The original counts were made by Potzger, but before the samples were dried and fused by Deevey a comparatively large subsample (several cubic millimeters) was removed from each sample and stored. The subsampling must have been entirely random, as there was no way of telling which side or end of the somewhat dried chunks was originally uppermost in Potzger's borer. Later, when the necessity of examining the variance was more fully appreciated, the subsamples were homogenized by boiling them in KOH, five slides were prepared from each, and Potzger counted 50 grains in each slide for a total of 3000 grains on 60 slides. The means and standard deviations of the principal pollen components are given for each of the 12 samples in table 4.

The program of sampling was not such as to include a large fraction of the stratigraphic variance within any one sample, and all that inspection suggests is that the subsamples are stratigraphically equivalent. It also appears, from the fact that several of the means are somewhat discordant, that stratigraphic variance between samples, though negligible in the sense that all the samples belong to a high-pine zone, is not entirely suppressed.

Analysis of variance confirms the latter suspicion, for in the case of pine and spruce the error mean square between

samples is greater than the error mean square among counts, with significant F-ratios of 2.91 and 2.27, respectively; the other components do not depart significantly from homogeneity. Moreover, a negligible fraction of the variance proves to be binomial.

To test the significance of differences between the sample means of table 4 by means of t or F would be a burdensome task, and its logical basis would be somewhat questionable, as discussed above. The Kendall sum provides an easy, objective, though not perfectly efficient means of testing such differences, and no particular trouble is given by the fact that for any one pollen component there are $N(N-1)/2$ or 66 paired comparisons to be made. For two sets of pollen frequencies, in other words, for each of the five estimates of the frequency in each sample, one asks, "how many of the other set are higher? How many are lower?" Ties are split between the "higher" and the "lower" columns, the two columns are summed, and the smaller of the two is the Kendall sum. For sets of data where N_1 and N_2 are both 5, the 5% significance value is 2.85.

This method lacks the high efficiency of the Wilcoxon sum, discussed in a later section, and should not be relied on for critical comparisons, but it serves our purpose admirably. The 66 Kendall sums for sample-by-sample comparisons of pine frequencies are set out in table 5. The arrangement of the table is in order of rank in respect to mean pine frequency, and only significant Kendall sums are entered, so that one can see at once that closely ranked samples do not differ significantly from each other, whereas the samples highest in pine pollen differ from those lowest in pine pollen with a high degree of probability. Had sample 11 been rejected on such evidence, all but three of the significant differences would have been avoided; perfect agreement within the limits of efficiency of the Kendall sum would have been achieved by rejecting six out of twelve samples, numbers 10, 8, 7, 1, 2, and 11.

No great harm appears to have been done to the radiocarbon dating program by the inclusion of the discordant samples in the total. The standard pollen diagram for this locality is shown in figure 1, and it is plain that stratigraphic differences are exceedingly sharp in this profile. A discrepancy

in mean pine frequency between 69.2 and 84.0 per cent is not to be taken seriously as a departure from the high-pine zone. Moreover, the most aberrant sample, number 11, shows the highest pine frequency and the lowest spruce and fir frequencies, so that it appears to have been the youngest of the lot. Its inclusion therefore is not to be regarded as contamination, for it would be more correct to say that the other, older samples contaminated number 11. One might thereby explain a radiocarbon age for the pine zone at this locality that is somewhat greater than originally anticipated. On

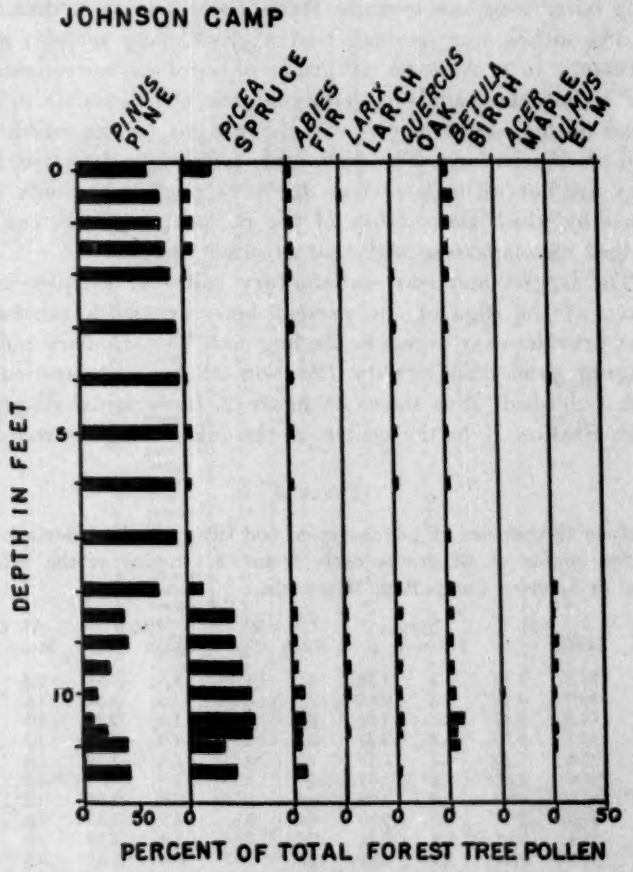

Fig. 1. Pollen diagram for Johnson Camp Bog, Minnesota.

the other hand, the aberrant pollen counts may merely have come from nearer the tops of their respective samples than the others, and thus we cannot be sure that there are real differences in average age among any of the samples.

THE HOMOGENEITY OF MULTIPLE SAMPLES: UPPER LINSLEY POND

The early work on Upper Linsley Pond, North Branford, Connecticut, was carried out, as had been stated, in ignorance of the seriousness of the problem of stratigraphic variance. The borer used, an oversize Davis borer having a diameter of 1⅛ inches, was so small that a great many samples were necessary to provide the 20 grams or so of carbon necessary for radiocarbon analysis; study of the stratigraphic differences within each sample was thought to be impossible as well as unnecessary. The data are accordingly defective, but they are not without interest, for they provide the only evidence by which the validity of the radiocarbon dates can be judged in comparison with that of other samples.

The largest and least satisfactory suites of samples were taken at the edge of the present lake, around a causeway that provides easy access to the bog mat. The standard pollen diagram from this locality (Station 2) has not previously been published; it is shown in figure 2, the original diagram from Station 1, in the center of the lake, being reproduced

TABLE 4

Mean frequencies of pollen types, and their standard deviations, in five counts of 50 grains each, from 12 samples at the 8-foot level in Johnson Camp Bog, Minnesota.

Sample Number	Pine		Spruce		Fir		Birch		All Others	
	Mean	σ	Mean	σ	Mean	σ	Mean	σ	Mean	σ
1	39.6	1.67	4.2	1.79	2.2	1.30	1.6	0.89	2.4	1.14
2	40.8	3.49	4.2	2.95	2.6	1.95	1.2	1.30	1.2	2.18
3	37.8	3.77	6.0	1.87	2.6	1.67	1.8	2.18	1.8	1.64
4	37.4	2.70	8.2	3.27	2.2	1.64	1.0	0.71	1.2	0.84
5	39.4	2.70	6.2	1.79	1.2	0.84	1.8	1.79	1.4	1.52
6	37.6	2.51	7.4	2.51	2.4	1.34	1.6	1.14	1.0	1.22
7	36.6	0.89	6.8	2.39	0.8	1.30	2.6	1.34	3.2	1.30
8	36.6	2.41	7.0	2.00	0.4	0.55	3.2	1.30	2.8	1.30
9	36.4	3.61	7.6	2.41	2.6	1.14	1.4	1.14	2.0	1.58
10	34.6	2.07	7.4	2.07	2.0	1.87	3.0	1.41	3.0	0.71
11	42.0	1.41	3.6	1.14	0.4	0.89	1.6	1.14	2.4	1.34
12	38.2	3.35	5.8	1.64	1.6	1.14	1.4	0.55	3.0	1.41

as figure 3. On several scores, the greater depth of sediment, the convenience of access at all seasons of the year, and the perfection of correlation (in the usual pollen-analytical sense) with profiles at Station 1 and elsewhere in eastern United States, Station 2 appeared a thoroughly satisfactory place to undertake sampling for radiocarbon. Fifty-six samples were taken at 10.15 m. in the pine zone, 59 at 8.65 m. in the middle of Zone *C-1*, 70 at 6.65 m. in the transition from Zone *C-1* to *C-2*, and 62 at 4.65 m. in the middle of Zone

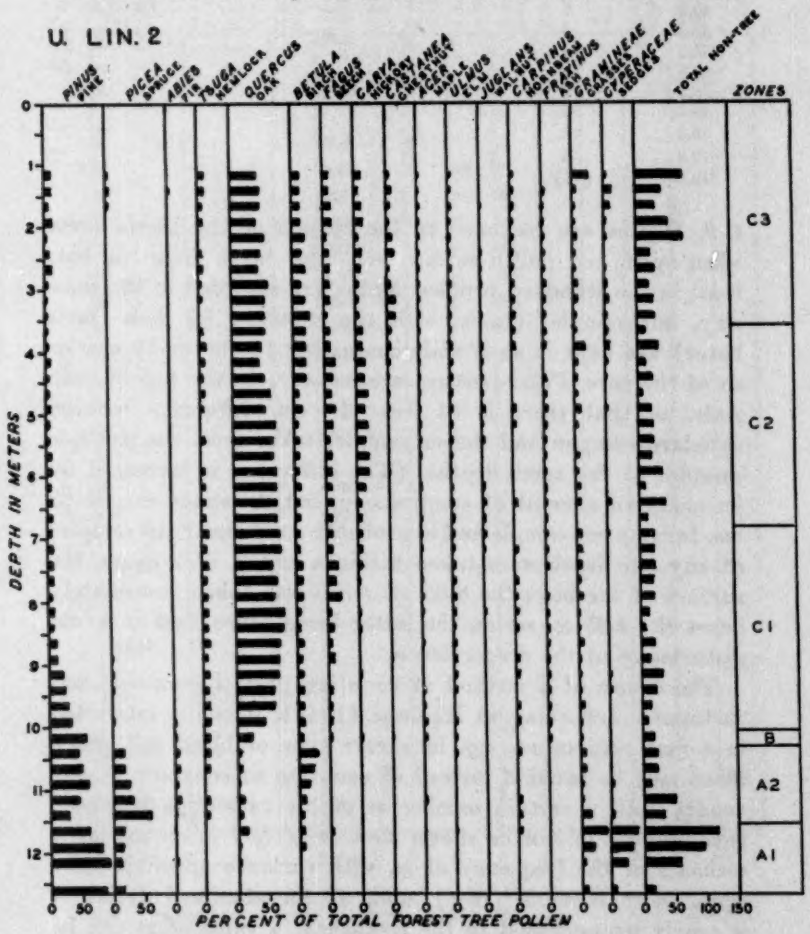

Fig. 2. Pollen diagram for Station 2, Upper Linsley Pond, Connecticut.

TABLE 5

Kendall Sums for paired comparisons of pine frequencies in samples from the 8-foot level in Johnson Camp Bog, Minnesota. Arranged in order of frequency as listed in table 4. Sums are entered for significant differences only.

Frequency	Sample No.	9	8	7	4	6	3	12	5	1	2	11
34.6	10	—	—	—	—	—	—	—	1.5	0.5	0.5	0
36.4	9		—	—	—	—	—	—	—	—	—	1.5
36.6	8			—	—	—	—	—	—	2.5	—	0.5
36.6	7				—	—	—	—	—	1.5	1.5	0
37.4	4					—	—	—	—	—	—	0.5
37.6	6						—	—	—	—	—	2.0
37.8	3							—	—	—	—	2.5
38.2	12								—	—	—	—
39.4	5									—	—	—
39.6	1										—	—
40.8	2											—
(42.0)	(11)											

C-2. Depths are reckoned to the bottom of the Davis borer when open, and pollen smears were also taken from the bottom; in the standard profiles depths are recorded in the same way, but samples (taken with the standard ¾ inch Davis borer) are kept in shell vials containing the lower 10 cm. or so of the core. Pollen smears are taken from the top of such vials, so that there is at least 10 cm. difference between standard samples and the subsamples taken from the multiple samples at the same depths. (The difference is increased by an unknown amount of compression, but it cannot exceed 25 cm. for any one sample and is probably much less.) All samples at any one depth were taken within a radius of 2 m. on the surface of the bog; the 8.65 m. series was taken immediately below the 4.65 m. series, the latter being taken first to avoid disturbance of the deeper levels.

The choice of a method of counting proved somewhat unfortunate. According to Haldane (1945), if one is interested in a rare component, *e.g.* in a rare type of blood cell, much labor may be saved if instead of counting to constant N, one counts until a certain number m of the rare type has been encountered. It can be shown that $m\text{-}1/\ N\text{-}1$ is an unbiased estimate of the frequency of m, with variance approximately equal to $m(N\text{-}m)/N^2(N\text{-}1)$, and, as the standard deviation is nearly proportional to the frequency, a value of m can be

chosen so that the standard deviation will always remain at some desired fraction of the frequency, such as one-tenth. Unhappily, in pollen work it is impractical to choose a value of m that will achieve this small ratio of σ/m. Moreover, the assumption that the values of N so determined for a suite of

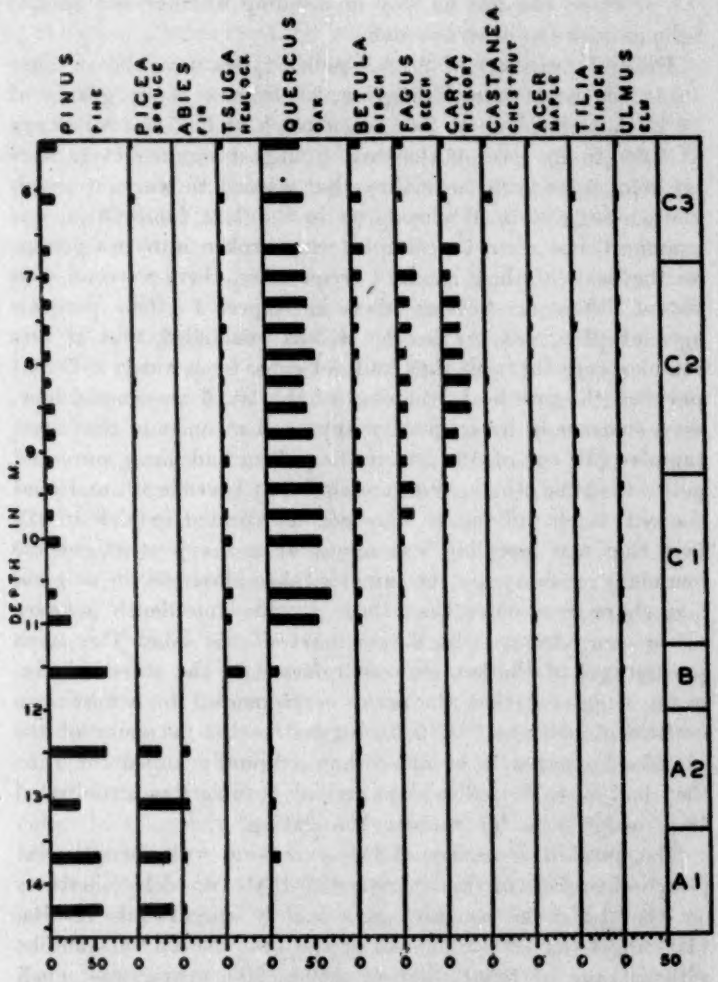

Fig. 3. Pollen diagram for original boring (Station 1), Upper Linsley Pond, Connecticut. From Deevey, 1943, fig. 2. (courtesy American Journal of Science.)

samples at the same depth should be normally distributed if the samples were equivalent is now seen to have been naive; a normal distribution may in fact be expected, but the method provides no criterion of the significance of departures from normality, and as the binomial variances are large, σ/m being 0.5 or more, one has no way of deciding whether any sample belongs with the others or not.

Different numbers of "rare" pollen types were chosen after study of the standard diagram, as follows: 4 fir grains at 10.15 m., 4 hemlock at 8.65 m., 9 beech at 6.65 m., 8 hickory at 4.65. In the case of the three youngest samples there were no departures from normality that seemed to warrant suspicion; a suggestion of bimodality in the data for 8.65 m. was examined, but when the samples were broken into two groups on the basis of their hemlock frequencies, there were no consistent differences between them in respect to their percentages of pine, oak, or beech. It was concluded that if any samples were aberrant they had not come from widely different parts of the profile. In the case of the 10.15 m. sample, however, evidence of heterogeneity appeared at once, in that some samples (10 out of 56) had no fir pollen, had much more oak pollen than the others, and were obviously lower in silt and sand content. Such differences were not unexpected in view of the fact that the sampling was aimed at a sharp stratigraphic boundary; presumably the samples taken closer to the original lake shore were older than those at the same depth a meter or so away toward the deeper part of the lake. The mean percentages of the two groups, referred to the standard diagram, suggested that the error corresponded to a maximum vertical displacement of 0.25 m., and as the majority of the samples appeared to be older than originally hoped for (*i.e.*, they had more fir pollen), no serious trouble was anticipated from using them for radiocarbon dating.

The unrealistic nature of this reasoning was abruptly exposed when Doctor Libby reported that two determinations on the 10.15 m. sample gave widely discordant results, 7100±250 and 4200±250 years, and that the 8.65 m. sample, with an age of 6800±300 or 4400±300 years, was much younger than had been supposed. Subsequent sampling at Station 2 has made the reason for the discrepancies perfectly

clear. Obstructions at various levels, notably between 8 and 9 meters, have proven to be wood of fresh appearance in all instances where samples of them could be obtained, and it is evident that in this bog-margin locality younger wood has repeatedly sunk down from higher levels to confound the stratigraphy.

It is remarkable that no evidence of this disturbance appears in the standard pollen diagram, but it must be remembered that such standard borings are made in single holes less than two inches in diameter, not over a circle of 2 m. radius, and, further, that if fragments of wood appear in peat borer samples they are avoided when the pollen preparations are made. If the stratigraphy of the sediment matrix is not violently contorted by the sinking wood, and if ordinary pollen counting practice accepts standard errors so large that no two samples one-quarter or one-half meter apart can be said to be significantly different, the fact that difficulties of this sort can escape notice should not be surprising.

Radiocarbon dates from Station 2 must therefore be rejected as based on non-homogeneous samples. Somewhat wistfully, we give in table 6 the indication that the two younger samples, at 6.65 and 4.65 m., must be included in this decision. The table shows that practically all the pollen types from all the samples differ from homogeneity, *i.e.*, from binomial variance, with highly significant values of chi-square. The two younger samples, however, are notably more homogeneous than the others, and the 4.65 m. sample, in which only the relatively low-ranking beech departs significantly from homogeneity, may be regarded as a doubtful case.

Difficulties of the sort experienced with the multiple samples of Station 2, however, are not inherent in the method; it seems certain that they reflect stratigraphic complexities peculiar to a locality near the shore of a bog lake. The samples taken in 1948 at Station 1, in the center of the lake, prove to have been reasonably satisfactory. The oversize Davis peat borer was used here also, 31 to 32 samples having been obtained at each of three levels through 10 or 11 holes in the ice. The holes for any one composite sample were scattered in a rough circle less than 4 m. in diameter, and separate sets of holes were used for the three different samples. All are

TABLE 6

Chi-square tests of homogeneity on multiple samples from Upper Linsley Pond, Station 2

		Mean Frequency %	χ^2	Degrees of Freedom	Probability of Null Hypothesis
10.15 m. (high-fir samples only)	Pine	49.1	95.05	45	<0.0001
	Oak	4.8	38.44	45	
	Spruce	13.6	127.82	45	<0.0001
	Birch	18.1	70.76	45	0.014
8.65 m.	Pine	8.3	87.99	58	0.011
	Oak	69.4	85.35	58	0.018
	Beech	6.1	56.08	58	
6.65 m.	Pine	3.2	106.09	69	0.010
	Oak	66.5	95.02	69	0.036
	Hemlock	4.1	59.61	69	
	Birch	3.9	82.01	69	0.267
	Hickory	5.6	88.35	69	0.110
4.65 m.	Pine	4.6	63.62	61	0.772
	Oak	52.7	79.27	61	0.110
	Hemlock	1.6	51.12	61	
	Beech	13.1	92.51	61	0.009
	Birch	6.2	57.98	61	

believed to have been within a few meters of the original boring made in 1940, and the agreement of the mean pollen percentages with those from the standard profile indicates that the depths of sampling were correctly chosen for Zones *B*, *C-1*, and *C-2*, and that no gross differences in stratigraphy are to be found within short distances in this part of the lake.

Sixty pollen grains were counted from each of the samples, with results that were so consistent (on inspection) that a false complacency about multiple sampling was engendered. The homogeneity is verifiable by the appropriate statistical test, as shown in table 7, the variance materially exceeding the expected binomial variance in the case of only two low-ranking pollen types among the fifteen tested. With proper attention to the realities of sedimentation in small lakes, therefore, the multiple sampling method can yield consistent results.

It should be remarked, as a final confession of ignorance, that two or three of the samples from each level at Station 1 were excluded from the composite samples on the basis of apparent inconsistencies in pollen frequencies that are now known to have been meaningless. A few more were quantitatively negligible because of improper performance of the borer in the field, and these may contribute to the variance expressed

in the calculations of table 7 even though their carbon content does not contribute to the dates of the composite samples. The result was that the total material provided to Doctor Libby from Station 1 ranged from barely sufficient to insufficient. A more elaborate program of pollen counting, such as that applied two years later to the samples from Lake Cicott Bog, would have provided an objective basis for the rejection of aberrant samples, but the quantity of sediment was inadequate to begin with, and the plain fact is that the small borer is not a suitable instrument for multiple sampling.

STRATIGRAPHIC DIFFERENCES IN A NORMAL POLLEN PROFILE

We turn now from the problem of the stratigraphic equivalence of multiple samples at the same level to its inverse, that of stratigraphic difference between closely spaced levels. The data we can bring to bear on this matter are by no means comprehensive, and there is room for much more incisive and thorough study, but Faegri and Ottestad (1949) gave a full discussion, and we are able to comment on their recommendations on the basis of independent evidence.

TABLE 7

Chi-square tests of homogeneity on multiple samples from Upper Linsley Pond, Station 1

		Mean Frequency %	χ^2	Degrees of Freedom	Probability of Null Hypothesis
11.65 m.	Pine	26.56	35.92	31	0.509
	Oak	55.78	41.11	31	0.208
	Hemlock	3.59	26.16	31	———
	Birch	6.77	41.71	31	0.184
	Beech	2.40	97.93	31	<0.0002
9.15 m.	Pine	3.49	24.14	30	———
	Oak	65.22	25.05	30	———
	Birch	5.54	21.85	30	———
	Beech	6.56	24.44	30	———
	Hickory	12.58	25.44	30	———
8.05 m.	Pine	3.66	27.72	30	———
	Oak	58.06	27.61	30	———
	Birch	5.86	51.30	30	0.014
	Beech	10.86	29.57	30	———
	Hickory	13.60	40.07	30	0.204

When the 8.65 meter composite sample from Upper Linsley Pond, Station 2, proved to look somewhat heterogeneous, we were forced to consider the question, whether we could actually

tell the difference between two levels of the standard profile (separated by 25 cm.) in a zone where the frequencies of the various pollen components are so nearly alike. Considering the samples at 8.40, 8.65, and 8.90 meters in the standard diagram, where the pine frequencies (based on counts of 150 pollen grains) are 4.7, 8.0, and 10.0 per cent, respectively, two points are evident. With respect to the postglacial section as a whole, this half-meter zone shows a long-term trend of decreasing pine pollen. On the other hand, with binomial standard deviations of the order of 2.5 per cent, and 95% confidence limits of the order of 10 per cent ($\pm 2\sigma$), it is equally clear that the differences are not statistically significant. The pine frequencies happen to fall in the right order in these three samples, but the beech frequencies, for instance, do not, as they are 4.7, 2.7, and 5.3 per cent, respectively, even though the rest of the diagram tells us that beech ought to have been increasing with time throughout Zone *C-1*.

We have discussed the variance to be expected at any one level, and have pointed out that while various kinds of sampling error may perhaps influence the results of pollen counting, all the available evidence points to the conclusion that binomial variance alone will account for the range of frequencies encountered under ordinary circumstances. Most of this evidence was given by Faegri and Ottestad from their own work and from their analysis of data published by Woodhead and Hodgson; we have added to this evidence by treating the data published by Bowman, and have supplemented it by the demonstration that where there are no gross disturbances of stratigraphy, as in the center of Upper Linsley Pond, whole suites of samples of finite length can be taken, subsampled almost at random, and analyzed without adducing any significant departure from binomial variance. Such variance of course is large, and very large total numbers of pollen grains must be counted to reduce it to manageable proportions, but so long as it can be managed the more difficult part of the problem is not beyond solution.

When two successive samples in the same profile are considered, if the estimates of pollen frequencies within samples are subject to strictly binomial distribution of error, real differences between samples can be regarded as entirely strati-

graphic, and the task is to estimate how far apparent differences are likely to be real. This is not a difficult matter at first sight, for obviously one can show that even small differences between frequencies are probably real if he counts enough pollen grains to make the errors of estimate negligible. However, stratigraphic differences between samples are of two sorts, as Faegri and Ottestad pointed out: in addition to the sweeping changes of pollen frequency that imply vegetational development and climatic change, and in which the pollen worker is mainly interested, there will be fluctuations of a less meaningful kind that are nevertheless real in a stratigraphic sense. Year-to-year shifts in the relative intensity of pollen production by the nearby vegetation, minor variation in the composition of the pollen rain that ultimately reaches the sediments at a given point, and seasonal differences in pollen frequency, as between spring- and summer-flowering species, are among the possible causes of such unimportant oscillations. Faegri and Ottestad speak of them as "short-term" fluctuations, in contradistinction to the more interesting "long-term" changes. It is reasonable to suppose that short-term stratigraphic variance will show up in samples taken a centimeter apart or less, while long-term changes might be detectible within 5 or 10 centimeters, but this quantitative aspect of the theory of stratigraphic variance is the part that is most in need of empirical documentation. At any rate, the ideal sampling program must take into account the existence of short-term as well as long-term differences, and permit their estimation from the data.

Our program of sampling around the 8.65 meter level, though far from ideal, has led to illuminating results. The "experiment" was carried out on the 8.65 m. sample from the standard profile at Station 2. It was about 10 cm. long, and represented somewhere between 10 and 25 cm. of the original deposit. We removed the sample from the hermetically sealed vial in which it had been stored, and prepared 25 different pollen slides from the region of the top, in addition to 25 slides from the bottom. The total material removed for 50 preparations was appreciable, but not more than 10 per cent of the volume of the whole sample.

Counts of pollen grains were then made by the method of

Haldane (1945), choosing an arbitrary number (6) of a rare species (hemlock) so that the average total number of grains counted would just exceed 100. The total numbers counted on 25 slides were 2658 and 2579, but as the percentages calculated for each pollen component are based on *N-1*, the totals corrected for bias are 2633 and 2554. The means and standard deviations of the various pollen components are given in table 8.

Before we examined the significance of differences between the "top" and the "bottom" series, we thought it advisable to test the samples at one level for homogeneity. Because the variances of some of the frequencies seemed distressingly large, we tried the effect of repeating the counts on different parts of the same slides. Only the "top" series was duplicated in this way, and the results are less interesting than they

TABLE 8

Mean pollen percentages in upper and lower parts of a single peat borer sample (Upper Linsley Pond, Station 2, 8.65 m.), and their standard deviations, based on counts of 25 separate subsamples at each level.

	"Top"		"Bottom"	
	Mean	σ	Mean	σ
Oak	72.84	5.21	71.03	5.14
Pine	6.65	3.00	11.39	4.25
Hemlock	4.75	3.05	4.89	2.67
Beech	4.82	2.68	2.35	1.74
Birch	4.44	2.14	4.35	2.23
Elm	1.90	1.45	2.51	1.97
Others	4.60	2.07	3.41	1.85

might have been. The first counts were made by using the mechanical stage in the usual way, beginning at the lower edge of the cover slip and traversing the slides in a series of horizontal strips one microscope field wide, with a small "safety" space separating each strip from the next. A sufficient number of pollen grains was reached well before the mid-line of any one cover slip. Had we then moved to the mid-line and continued the count toward the other edge of the cover slip, we might have found differences of the sort Wenner (1948) has emphasized, those produced by non-random movements of pollen grains through the smear. Instead, we turned the slides upside down and approached the mid-line from the

upper edge of the cover slip. The differences in the two sets of counts may seem astonishingly great to the average pollen worker, even though they reflect little but binomial variance. The frequencies of hemlock grains, for example, range in one series from 2.89 to 14.3 per cent, and in the other series from 2.12 to 9.80 per cent, while the two sets of hemlock frequencies are completely uncorrelated, with a correlation coefficient of 0.173. At the same time, the differences can easily be proved to be unsystematic, and the method used to show this, the Wilcoxon sum, will be briefly described here because other workers may find it useful.

The Wilcoxon sum (Wilcoxon, 1945, 1947, 1950) is used for paired data, and gives a method of testing significance of differences that is both simple and highly efficient, particularly on small samples. Differences between members of pairs are set down, ranked according to magnitude without regard to sign, then the rank values are segregated according to sign and summed. The smaller sum is the Wilcoxon sum, whose significance is then looked up in a table. If, among eight sets of paired variates, the smallest and the next smallest differences are negative, and the remainder are all positive, the Wilcoxon sum is 3, and the two sets differ from each other to the 5% level of probability. In our case, with 25 pairs of hemlock frequencies, the Wilcoxon sum of 144 is entirely insignificant, and while this is a trivial application of so powerful a method it will suffice to illustrate its use.

Finally, chi-square tests of homogeneity were applied to both sets of estimates of hemlock frequency from the "top" group of samples, with the expected result that neither series differs significantly from its own mean binomial variance. The values of χ^2 were 28.49 and 28.69, each with 24 degrees of freedom. The probabilities that these values could have been obtained by accident are about 0.3 in each case, but the positive differences between χ^2 and the number of degrees of freedom suggest that some extraneous variation is present, unimportant though it may be. The source of this extraneous variation is presumably to be found in stratigraphic fluctuations of a short-term variety.

Returning to table 8, despite the great similarity of pollen composition between the "top" and "bottom" groups of sam-

ples, the lower part of the original sample is richer in pine and poorer in beech than the upper. These are the differences expected from the (non-significant) long-term trend visible in the pollen diagram (fig. 2); their reality is adequately demonstrated by analysis of variance.

In carrying out analysis of variance on these percentages, the question of weighting arises, as each count was terminated at a different total number. For a single classification it would be comparatively simple to employ binomial weighting (Cochran, 1943) in which each percentage is weighted according to the total number on which it is based. In this case, however, we are interested both in the variance between groups (long-term stratigraphic variance) and in that within groups (short-term stratigraphic variance), and we have a twofold classification, in which weighting procedures become excessively cumbersome. As the appropriateness of analysis-of-variance technique for data that are distributed in binomial or other non-normal fashion is debatable at best, we have thought it fair to employ equal weighting, and to regard the results with our customary suspicion.

According to the analysis given for pine percentages in table 9, pine shows a clear long-term trend of decrease between the bottom and the top groups of samples, while the short-term stratigraphic variance within samples is negligible compared to the residual (presumably binomial) variance of counting. The *F*-ratio for the "between groups" and the "within groups" mean squares, 21.25, appears highly significant. Closely similar results were obtained for the beech percentages, which thus show a clear long-term trend of increase with time. The other pollen components show no significant differences, with the possible exception of the "all others" component, which is virtually meaningless stratigraphically.

Is it legitimate to conclude that pine and beech really underwent significant changes within the segment of time represented by about 10 cm. depth of deposition? We believe it is, with some reservations. The differences appeared to be consistent, even while the counts were being made; the *F*-ratios are so large that it is difficult to believe that they merely represent disturbances due to non-normality of the data; employment of equal rather than binomial weighting probably

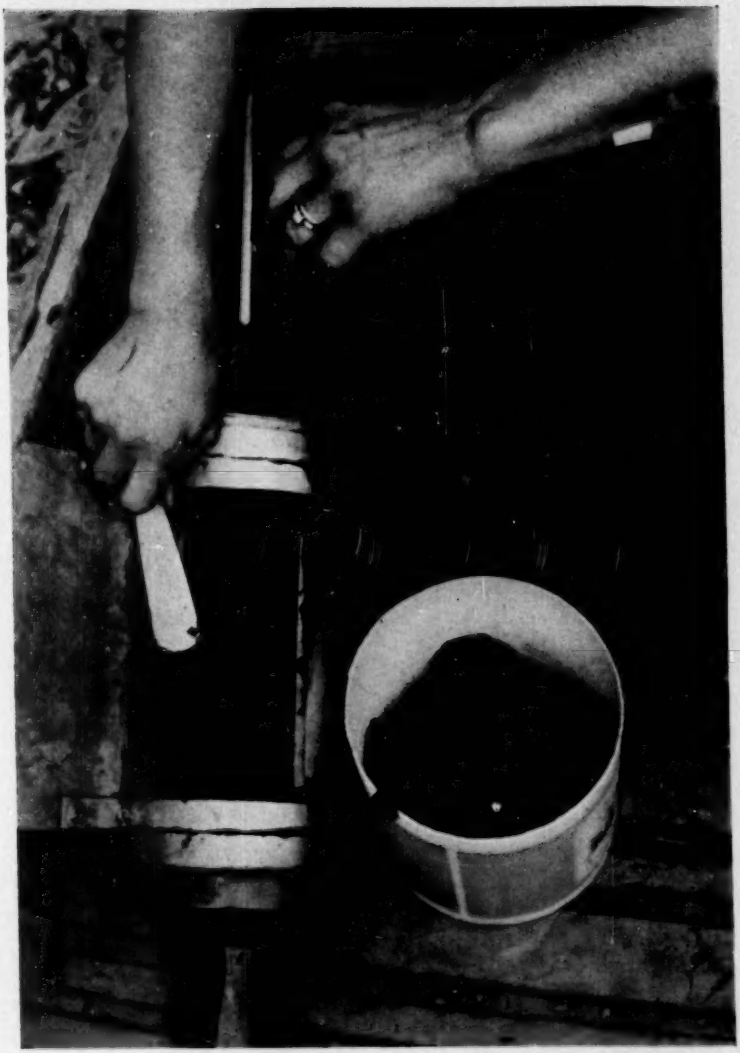

Plate 1. The 6-inch Hiller sampler, constructed especially to obtain large amounts of sediment for radiocarbon analysis.

results in underestimates rather than overestimates of significance. On the other hand, we dare not regard the matter as closed, for we need not merely additional empirical data from pollen counting, but more extensive investigation of the mathematical basis of analysis of variance, a valuable tool but one that can be misused.

TABLE 9

Analysis of variance, and estimation of components of variance, for pine frequencies in "top" and "bottom" of a single peat borer sample (see table 8).

	Degrees of Freedom	Sum of Squares	Mean Square	Estimate of:
Total	49	891.4796		
Between groups	1	248.4983	248.4983	$\sigma_C^2+25\sigma_A^2$
Within groups	24	274.9603	11.4566	$\sigma_C^2+\sigma_B^2$
Residual	24	373.0210	15.5425	σ_C^2

Variance between groups	$= \sigma_A^2 =$	9.1182
Variance within groups	$= \sigma_B^2 =$	-4.0859
Residual variance	$= \sigma_C^2 =$	15.5425

Thus it is possible, if one is willing to count something like 2500 pollen grains instead of 150 at each level, to demonstrate stratigraphic differences between closely spaced levels of a pollen profile. A smaller number, of the order of 1000, would almost certainly suffice, but as we now regard Haldane's method of counting as unsuited to pollen work, we have not troubled to calculate the minimum number from our data. As the differences demonstrated to be real exist within a zone of rather great uniformity, it is legitimate to expect equally decisive results in segments of profiles where rates of change are greater.

We can now discuss the contribution of Faegri and Ottestad to this problem. Their approach to the matter of short-term and long-term stratigraphic variance was essentially the same as ours, but their sampling program was more extensive and less intensive than our own. Instead of a number of subsamples at the same level in a borer sample, they took only two, presumably 1 cm. apart, though the distance was not explicitly stated. They compared "pairs" of samples taken in this way with other "pairs" at 10 or 20 cm. intervals.

Although they were unable to demonstrate long-term stratigraphic variance between two pairs by convincingly high values of the *F*-ratio, particularly in view of the difficulties of interpretation entailed by borderline values of significance, the whole series (60 cm. long) gave definite results. They therefore proposed that future pollen workers follow their program. That is, they suggested that when, in the course of routine work, samples are taken 5 cm. apart, an "extra" series should be taken 1 cm. from the first set. Analysis of variance between "pairs" and within "pairs" should then show the effect of short-term fluctuations and permit the testing of the significance of long-term changes. As one can get at least as much information by counting N pollen grains in two samples as by counting $2N$ grains in one sample, the additional labor would be restricted to the taking and preparation of the second, "extra" series.

Although the reasoning behind this program is sound, we believe that one degree of freedom for error in comparisons within "pairs" is too few, and while we do not imply that it will always be necessary to take 25 samples at the "same" level, some intermediate degree of replication is desirable for the control of short-term stratigraphic variation. A figure of 10 replications is suggested as reasonable; counts might be limited to 50 pollen grains in each sample, but we doubt that pollen workers will be willing to count even 500 grains at each level unless some exceptionally interesting problem is under consideration.

Pollen stratigraphers who follow a sampling program such as that suggested here must be aware that analysis of variance is full of pitfalls, and that its application to percentages is especially treacherous. We therefore hesitate to recommend its use so warmly as do Faegri and Ottestad. Instead we suggest that when several groups of (say 10) samples taken at the same level are compared with each other, the comparison be made by means of Kendall sums. Alternatively, short-term variations can be eliminated by taking larger samples, say 1 cm. slices through the entire core, and homogenizing them before subsamples are drawn. In all likelihood, a total count of 500 grains at each level will still turn out to be the minimum number necessary for demonstration of long-term changes between successive levels.

THE LARGE SAMPLER

No reader who has followed our struggles to obtain large quantities of material for radiocarbon dating by means of multiple samples can have failed to wonder why we have not circumvented the problem altogether by making the sampler larger. We have in fact attempted to do so, and plate 1 shows the apparatus. It is a brass sampler constructed on the Hiller model, with a diameter of 6 inches and a slit opening 10 inches long. The drill point is made of hard wood, to reduce the weight, and the extension rods, which must be rigidly articulated in order to permit opening and closing the sampler at depth by twisting, are greatly reinforced at the joints and held together by two sets of steel screws. Several samples have been obtained by this means at a saving of time and effort that can be appreciated only by those who have read the preceding pages. Unfortunately the instrument is so unwieldy that it has been impossible to use it below a depth of 6.5 meters in the mud.

At times in this investigation we have thought that it might be simpler in the end to buy Upper Linsley Pond, drain it, excavate it with a bulldozer, and take our samples from the cut face of an excavation by means of a spade. But while this might be possible for one locality, it would hardly be possible for several, and we have preferred to leave our bogs essentially intact, hoping for the time when Doctor Libby's method will be refined to the point where single peat-borer samples can be utilized for dating purposes.

CONCLUSIONS

The problem of estimating the age of pollen-bearing material by means of its pollen content is a complicated one. Routine pollen stratigraphy involves the removal of microscopic amounts of material at intervals from a vertical section obtained with a peat borer. Many pollen types are represented, and estimates of the frequency of even the most numerous types are subject to large variation of a binomially distributed kind. The usual practice of counting about 150 pollen grains at each level is defensible so long as it is realized that small differences between frequencies at adjacent levels have no statistical significance. When major shifts of vegeta-

tion are recorded by successive pollen spectra, as when a pine zone overlies a spruce-fir zone, the pollen worker does not need to be a statistician to observe and interpret the difference: when such shifts are repeatedly found in pollen profiles obtained from widely separated localities, there can be no question of their validity. Small differences within fairly homogeneous pollen zones, however, cannot be considered to be real without statistical proof, and this conclusion applies to such conventions as the one, widespread among European workers, of drawing zonal boundaries at "rational borders," *i.e.*, levels where some minor pollen component, previously "constant" around 1 or 2 per cent, begins its "steady rise" toward values of 8 or 10 per cent.

Such a practice is patently absurd if the percentages are based on total counts of 150 or so, but while small differences can be shown to be real if they hold up after counting much larger numbers, this procedure does not in itself solve the problem. Because there may have been year-to-year or seasonal differences in pollen sedimentation, it is perfectly possible for two successive samples to give sharp differences that are entirely reliable, without proving that they mean anything in terms of the succession or development of vegetation. Such essentially meaningless fluctuations have been termed "short-term" changes by Faegri and Ottestad. They can be allowed for by taking several samples at the same level for pollen preparations. For a first approximation, the significance of the "long-term" changes assumed to have taken place between successive samples can perhaps be estimated by comparing variances among such replicated counts at two levels; such a comparison is most simply made by means of a Kendall sum. Replicated samples might be taken within a vertical distance of 1 cm. when successive samples are taken at intervals of 5 or 10 cm., as Faegri and Ottestad suggested; the Norwegian authors recommended duplication, but we believe that two samples will generally be found insufficient for the control of short-term stratigraphic variance. A more elaborate procedure would be to apply analysis of variance, as outlined in any moderately advanced textbook of statistical methods, such as that of Snedecor (1946). Comparison of the variance within groups and between groups of such rep-

licate samples will provide significance tests for the long-term stratigraphic differences in which the pollen worker is chiefly interested. Caution is necessary in interpreting such tests, because the errors of percentages are not normally distributed, and disturbances of levels of significance obtained from tables may result. Where the differences in pollen percentages are small, we doubt that reliable results can be obtained unless at least 500 grains are counted at each level, 50 in each of 10 replicates, but the problem deserves a fuller study than it has yet received.

We have devoted a large share of our efforts to the solution of a special problem in pollen sampling, that of deciding, when several samples are taken at the same level with a peat borer, whether they are stratigraphically equivalent. This is the converse of the usual problem, in that instead of seeking stratigraphic differences within short intervals, we attempt to design our sampling so that we find none. Strictly speaking, it has no solution, since there may be stratigraphic differences between two sides of a core, even one of pencil thickness, and, as cores have finite length, differences between top and bottom are certain to be present. Faced with the task of obtaining relatively huge amounts of material for the calibration of the pollen chronology, we accepted the risk that age differences between the top and the bottom of cores might exceed the accuracy of determination of age by the radiocarbon method, and attempted to save time by resorting to multiple sampling. We endeavored to minimize age differences within cores by sampling localities having deep deposits, and we concentrated our efforts on pollen zones, such as the pine zone, which are comparatively thick, uniform, and well marked. We endeavored to minimize stratigraphic differences between samples by careful field work, making the circle of sampling small and the depth determination accurate.

The saving of time proved to be entirely illusory, and the only satisfactory solution to this special problem is to use a sampler that will take the requisite amount of material in one operation. Our best samples were in fact taken with such an instrument. The method of multiple sampling was employed, however, at depths beyond the limits of efficiency of the large sampler, with varying degrees of success, a fact that must

inevitably influence the interpretation of radiocarbon dates established in this way.

For the restricted set of conditions stated above, which have regard for the usual circumstances of deposition in small lakes and bogs, our results show that a sufficiently homogeneous group of samples can be obtained, and unduly aberrant samples rejected, if three conditions are met. (1) The samples must be comparatively large to begin with, so that there are not too many sample-by sample comparisons to be made. Potzger's 1¾-inch Hiller borer, six samples from which provided enough material for a radiocarbon date, is a far better instrument for the purpose than Deevey's 1⅛-inch Davis borer, and ordinary Davis or Hiller borers are totally inadequate. (2) The design of pollen counting must take into account stratigraphic differences within samples. Where these differences are not very great, their absolute magnitude is immaterial; it is essential to include them, however, in comparing variances between samples, for the test of equivalence, by the criteria we have accepted, is based on the homogeneity of variances. The Kendall sum is probably reliable as a test of such homogeneity, though we have not used it for critical comparisons. (3) There must be no gross disturbances of the deposits sampled. We have met with confounded stratigraphy only at Station 2, a bog-margin locality close to the original shore of Upper Linsley Pond, Connecticut, and it is reflected in unduly divergent pollen percentages among samples; unfortunately we have no estimate of the stratigraphic variance within samples at this locality. At least two, and probably all four of the composite samples taken there must be rejected; all others, including the three taken in 1948 in the center of Upper Linsley Pond (Station 1), appear to have been adequately homogeneous.

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Osborn Zoological Laboratory,
Yale University,
New Haven, Connecticut

Butler University,
Indianapolis, Indiana

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WATER IN PRIMORDIAL AND DERIVATIVE MAGMA AND ITS RELATION TO THE ORE-FORMING FLUID*

JOHN DREW RIDGE

ABSTRACT. It is suggested that the rocks developed in the primary crystallization of the earth were considerably higher in water, mainly as OH^{-1} ion, than are the igneous rocks of later origin now known at the earth's surface. The chemical considerations bearing on this point are discussed in some detail; the differences in the behavior of water in a rapidly and a slowly crystallized melt are brought out. The concept is advanced that a large excess of water in rocks of the primordial earth is the source of the water involved in the majority of ore depositions. The water is separated from the melt through the inability of a slowly cooling silicate magma, derived from the remelting of primary, water-rich rock, in the late stages of its crystallization cycle to retain in solution more than a small fraction of the water it originally contained.

WATER IN RELATION TO ORE DEPOSITION

IN the numerous papers since the days of Hutton and Werner, that have discussed the development of the ore-forming fluids, much has been said, and left unsaid, concerning the source and quantity of water they may contain. The positions taken by those who admit that water (as liquid, gas, or vapor) has been important in the deposition of most ore bodies have run the gamut between the two possible extremes: (1) that all the water of the ore fluid was of meteoric origin and (2) that all of it came from magmatic sources. Although Werner was a strong advocate of the deposition of ore materials from aqueous solutions, he believed that these were all low-temperature, near-surface liquids throughout their entire careers as ore carriers and ore formers. Only a very few geologists today would take so extreme a view but many attribute much of the ore fluid to additions of meteoric water to an original magmatic liquid containing considerable water. J. G. L. Schmidt, in 1822 (Crook, 1933, p. 41) seems to have been the first clearly to express the idea that hot waters from the earth's interior, with mineral material in solution, had deposited their loads "in fissures and cavities on their way to the surface in consequence of falling temperature." He has had many followers who have held and hold quite similar views.

In contrast to those who admit that water has been important and probably vital in the formation of ore deposits, there

* Contribution of the Division of Geology, The Pennsylvania State College.

has been a school which stems from Hutton's concept that the ore sulfides had been melted and transported in the molten state until they were cooled enough to recrystallize. Hutton did not believe that water was of any importance in the collection or transportation of ore or gangue minerals (Crook, 1933, pp. 35-36.) Hutton has had many intellectual descendents who, if they did not envision completely anhydrous ore magmas, relegated water to a very subordinate role in ore deposit formation. These men include Fournet in 1844, Belt in 1861, Morrow Campbell in 1921, Spurr in 1923 (Crook, 1933, pp. 48-54, 76-81, 117-122, and 123-127) and White (1941), and J. S. Brown (1948).

On those who advocate the importance and abundance of magmatic water in the ore-forming fluid lies the necessity of demonstrating that the supposed parent magmas of these fluids (and the rocks from which the magmas themselves were derived by remelting) contained water in sufficient quantity to transport the ores in question. If this cannot be done, then the case for a magmatic source of a water-rich fluid is seriously weakened and recourse must probably be made to meteoric waters or ore magmas or to a combination of both in varying degrees to explain ore mineral transportation.

WATER CONTENT OF MAGMAS

To determine the possible water content of the magmas from which ore solutions may have come, it is necessary to consider how much water could have been contained in the rocks from which the magmas were formed. In the analyses of igneous rocks given by Johannsen (1932, II, pp. 192-193; 1937, III, pp. 220-221) the water content of granite averages around 0.75% or a little less, while that of gabbros is probably somewhat over 1.00%. It appears that roughly two-thirds to three-quarters or more of this water is actually present as OH^{-1} and only a small fraction is there as H_2O ("water of crystallization"). It is difficult to determine if the magmas from which these rocks were formed contained more water than is locked up in their solidified products; any excess water over that usable in the rock minerals undoubtedly would have been driven far from the magma from which it came. The work of Goranson (1931, particularly, and 1932 and 1936), while apparently putting an upper limit on the amount of water that a granite magma may contain under certain conditions

of temperature and pressure, demonstrates that the water content of granitic magmas under sufficient confining pressure may be considerably above that found in the average solid granite. Some, though probably far from all, igneous magmas, therefore, may have been, but were not necessarily, markedly richer in water than the solid products which formed from them. The initial water content of any magma derived from the melting of rock is limited by the water content of its parent rock and by its (the magma's) ability to hold water once it is in the molten state. The magma may pick up some water if it rises high enough in the earth's crust to encounter water-bearing strata but the high water-gas pressure of the still fluid magma in such a near surface position would tend to prevent it. If the parent rock of magma is typical gabbro or granite or any gradation between (as known at the earth's surface), it follows that water in the magma probably never will amount to more than one per cent or a little more. If, on the other hand, the rocks which are melted to form magmas (or at least the magmas from which water-rich ore fluids come) are not the essentially anhydrous, intrusive igneous material of our shell of observation, but a more water-rich rock developed during the primary crystallization of the earth, the magma may contain considerably more water than the rock into which it eventually solidifies.[1] Before the effects of this concept of a water-rich primary solid earth can be discussed, it must be shown that the processes of earth solidification probably would produce primordial rocks much higher in water than the secondary igneous rocks of the known portion of the earth crust.

WATER IN THE COOLING MOLTEN EARTH AND ITS ATMOSPHERE

Any consideration of the water content of the solid primary earth must begin at the temperature at which water became available in the atmosphere in quantity. Nutting (1943, p. 123) quotes values for the degree of dissociation of water at various

[1] The possibility that similar, water-rich magmas might be produced by the melting of water-filled sediments is recognized. The thesis that the ore elements cannot be concentrated in economic amounts by igneous processes alone has been advanced by W. Q. Kennedy (1948) who suggests that only those magmas formed from sediments in which ore elements have been concentrated by sedimentary processes can produce fluids simultaneously water- and ore element-rich. It is beyond the scope of this paper for me to consider the validity of this concept.

temperatures which indicate that, at the pressures which must have prevailed in the cooling planet, water-gas should have been present and largely undissociated at 3500°K. At this temperature, the earth should have consisted almost entirely of liquid and gas, the bulk of the former having been a molten iron-nickel core, surrounded by a growing shell of molten of oxides and/or silicates, and the latter having contained everything else now in the solid earth, biosphere, hydrosphere, and atmosphere. At 3500°K, then, water would have been available for solution in the oxide melt or the silicate melt into which it probably was shortly converted. The partial pressure of water-gas in that primitive atmosphere almost certainly was high and water probably was forced thereby into this high temperature melt: (1) to the limit of its solubility or (2) until the pressure of water-gas in the melt equalled that of the water in the atmosphere. Because of the probably small solubility of water in the melt at this elevated temperature (the reasons for which are given in the next section of this paper), the former of the two alternatives probably obtained. As temperature fell and new increments of silicate shell were formed, water would have been extracted from the atmosphere to the extent made possible by the solubility of water in the melt. This progression of events probably would have continued uniformly until OH^{-1} ion became stable in the melt. Then the solubility of water in the melt would have tremendously increased (the reasons for this behavior also are given in the next section). This increase in water solubility probably was great enough that the partial pressure of water-gas in the atmosphere eventually was not sufficient to saturate the still growing quantity of melt. The amount of water in the melt, therefore, probably was no longer limited by the solubility of water in the earth-magma but by the ability of the water-gas in the atmosphere to hold it in solution.[2]

[2] As long as the partial pressure of water-gas in the atmosphere remained too low to force water into the melt to the limit of its solubility there, it made no difference how great the confining pressure was on the deepest portions of the melt in equilibrium with the atmosphere. Although an increase of confining pressure raises the solubility of water in silicate melts, no more water could have been dissolved in the primary magma than was equal (or approximately so) to the weight of water then in the atmosphere. Later in the crystallization history of the earth (as will be discussed shortly) when the amount of liquid magma had been considerably reduced, the content of water-gas in the atmosphere would have become sufficiently

By the time the temperature had dropped to 1500°K, the earth-magma presumably had reached, or very nearly reached, its maximum volume. Probably very little material, other than the substances today present in the atmosphere, would have been in the gaseous state. Almost all the elements not now free or combined in the atmosphere have some common exothermic compound (oxide, sulfide, silicate, or halide), the critical temperature of which is above 1500°K. They would, therefore, have been part of the liquid or solid portion of the earth by this stage in its history. The proportions of the constituents of the atmosphere at 1500°K (now either gases or vapors but then all gases) would have been very different than they are now. The amount of water in the atmosphere must have been much greater than that now in the oceans, on the land surface, in the biosphere, the atmosphere, and sedimentary rocks. Had it not been greater, the subtraction of the amount of water taken into the earth-magma between 1500°K and the temperature of final solidification would have resulted in an atmosphere, at the lower temperature, which would have contained only a small fraction of the water now on the surface and in rocks of surface origin. On the condensation (below 375°C) of most of this water-gas into liquid, the oceans would have been far smaller than they are now and could only have reached their present size through the addition of many times their initial volume of water from igneous sources.[3]

high in relation to that in the melt to have caused the melt to have been saturated with water. During this later stage, the solubility of water in the melt definitely would have been determined by the pressure of overlying liquid as well as by the pressure of water in the atmosphere.

[3] Several authors, among them H. Brown (1947) and Latimer (1950) have advanced the hypothesis that the earth's atmosphere and hydrosphere were both accumulated after the final development of the solid earth as by-products of igneous activity. I believe with Elasser (1950, p. 8) "that the assumption of a condensation of the earth from originally hot and fluid matter is so natural and consistent with all data that it is hard to circumvene." I also hold with R. T. Chamberlin (1947, p. 253) that the geological evidence favors the presence throughout earth history of an atmosphere and hydrosphere quite similar in amount and character to those known today. That water and carbon dioxide have been continuously made available at the earth's surface from juvenile sources in considerable quantity I have no doubt. Oxygen, however, I think unlikely to have been added in any large amount because *free* oxygen is far less soluble in silicate melts than water or carbon dioxide and because its presence in igneous emanations probably results only from oxidation-reduction reactions rather than from the relatively simple physical changes which drive H_2O and CO_2 from the melt.

By the time the temperature of the outer portion of the completely developed earth-magma had dropped to 1500°K, a considerable portion of the silicate mass adjacent to the core probably would have crystallized. As the temperature was further lowered, this interior solidified portion of the silicate shell would have grown larger outward and the amount of molten material in which water could be dissolved would have become progressively less. With each increment of the melt converted to the solid state (granting, as will be demonstrated later, that all or most of the water in each increment went into its solidified product) the amount of molten earth-magma would have become less and its total water content would have become less than that in the atmosphere. With the partial pressure of water-gas in the atmosphere thus greater than that in the reduced quantity of melt, the remaining volume of silicate liquid would have become increasingly saturated with water-gas until the limit of solubility under the conditions then prevailing was reached.[4]

Once this condition of water saturation obtained, any further reduction in earth-temperature would have done the following things: (1) increased the solubility of water in the melt available, (2) decreased the amount of melt (through crystallization of the then highest melting point minerals, and (3) changed the partial pressure of water-gas in the atmosphere. If each crystallizing increment of the magma would take over into the solid state all, or a considerable fraction, of the water it contained, this change in partial pressure always

[4] As will be shown in the following sections, most of the water would have held in the melt as OH^{-1}, formed by the reaction of H_2O and O^{-2}. The water present as OH^{-1} would not have affected the pressure or quantity of water-gas in the melt. That pressure would have been supplied only by the molecules of H_2O actually present as such in the melt and only by those within effective distance of the melt's surface. This follows because the pressure of water-gas in the melt would have been the result of the passage of water molecules from the melt to the atmosphere. Water molecules which were so deeply buried in the melt that such movement as they made never would have brought them to the surface cannot have affected the equilibrium in question. It must have been, therefore, only the water in a definite outer shell of the melt that maintained or attempted to maintain equilibrium with the water in the atmosphere. The depth to which dissolved water would affect the equilibrium with the partial pressure of water-gas in the atmosphere probably would depend on the rate of diffusion of water-gas in the melt, the solubility of water in it, the state of equilibrium between water molecules and O^{-2} on the one hand, and OH^{-1} on the other, and probably other factors as well.

would be to reduce it. The reasons for believing that an almost complete incorporation of the water dissolved in the magma into its solid product took place are presented in detail later in the paper (cf. pp. 527-528). The partial pressure of water-gas in the primordial atmosphere, then, would have been at a minimum as the last fraction of liquid in contact with the atmosphere solidified. The partial pressure of water-gas in the atmosphere at that moment should have been the weight of the following: (1) the water in the present-day oceans, (2) the water on the land surfaces, (3) the water in the biosphere, (4) the water in the atmosphere, and (5) the water in the sedimentary rocks. This Goranson (1931, p. 499) estimates to be $1.64 \times 10^9 km^3$ or $1.64 \times 10^{21} kg$ and, when all of it was in the atmosphere, the partial pressure of water-gas would have been about 305 atmospheres. From this, however, must be deducted an allowance for the water that has been brought to the earth's surface by igneous and volcanic processes since the final solidification of the earth's crust. If this deduction is assumed to be about 20 per cent of the $1.64 \times 10^{21} kg$, the final partial pressure of water would have been about 245 atmospheres (approximately 250 bars).[5] Such an approximation of the partial pressure of water-gas in the primordial atmosphere at the moment of final solidification of the crust does not, unfortunately, tell anything of the temperature at which that event took place. All that is relatively certain

[5] In a paper which appeared since this manuscript was initially submitted for publication, Ingerson (1950) has estimated the pressure of water vapor (gas) "at the temperature where crystallization would begin" to have been 330 kg/cm^2 or a little less than 320 atmospheres. As he makes no allowance for water which has reached the surface since crystallization began, the pressure calculated from Goranson's figures is in quite close agreement with his. Ingerson, however, takes no account of the reduction in water-gas partial pressure between the time crystallization began in the outer silicate shell (granitic is his designation of this shell) and that at which it was completed. In this he appears to have been mistaken because there was no such thing as a permanent equilibrium between the earth-magma and its atmosphere; the equilibrium constantly shifted in the direction of a lower partial pressure of water-gas. The figure of 320 atmospheres (245 by my estimate) is, therefore, that which obtained as the last, outer increment of earth-magma in contact with the atmosphere solidified and not that which held (according to Ingerson) when crystallization (presumably in the granite layer) began. The partial pressure of water-gas at the time crystallization began in the lowest portions of the granitic layer almost certainly was much higher and the percentage of water dissolved in the melt, therefore, probably was much greater than 3% (Ingerson's estimate of the average water content of primordial granite).

is that this partial pressure was considerably greater than 245 atmospheres even when only a few tens or hundreds of feet of the outer shell of earth-magma remained unconsolidated.

From the argument developed in this section of the present paper, it follows then that, during the crystallization of an important part of the outer silicate shell, the magma was surrounded by an atmosphere in which the partial pressure of water-gas was enough to insure the saturation of the melt with water to the limit of solubility obtaining at any temperature involved. The percentages of water in the rocks of perhaps the outer 50 km of the primary earth probably were of the order of magnitude of those determined experimentally by Goranson (1931) or perhaps even somewhat higher because of the tremendous confining pressure of overlying liquid on most of the crystallizing earth-magma.

WATER SOLUBILITY IN THE PRIMARY MAGMA

In the preceding section, it has been suggested that the molten earth probably was surrounded by an atmosphere that had sufficient total weight and a high enough partial pressure of water-gas to force the solution of water (at least in the outer crust) to the limit of the solubility of water in such a melt. Goranson's (1931) work, already cited, demonstrates that water can be dissolved in molten granite and sets the limit of such solubility far above the water content of presently known solid granites. It can be reasonably assumed that this large solubility of water may also obtain in the melts of other rock types. Goranson does not, however, show how the water dissolves in molten igneous material nor does he discuss the controls that govern the degree to which maximum solubility of water in such melts is attained in natural magmas.

To explain the chemistry of water solubility in the primary magma it is necessary to return to that temperature at which the first water appears in the gaseous portion of the earth mass (3500°K). Nutting's (1943, p. 123) values for the degrees of dissociation of water at various temperatures indicate, as has been pointed out earlier in this paper, that water-gas should have been present and largely undissociated in the primordial atmosphere at 3500°K. In other words, in the equilibrium

$$\frac{[H^{+1}]^2\,[O^{-2}]}{[H_2O]} = K, \qquad (1)$$

at 3500°K, the constant (K) should have been small and would have grown smaller as the temperature fell. Apparently then water would have been among the early compounds to form in the gaseous envelope and would have been available for solution in the molten portion of the earth mass. This water would, of course, have been far above its critical temperature and would not have been liquified until much later. The gaseous nature of this water, however, would not have prevented it from dissolving first in the initial oxide melt, which probably began to develop somewhat above or about the same temperature (3500°K), or later in the silicate melt to which the oxide melt, at least in large part, probably was converted. Barth and Rosenqvist (1949, p. 318) have stated that water is dissolved in silicate melts by the reaction

$$H_2O + O^{-2} = 2(OH^{-1}), \qquad (2)$$

with most of the H_2O accepted reacting with O^{-2} and only a small portion being present as actual H_2O molecules. It seems possible that this same reaction may have taken place to some extent in the initial oxide melt. Though water probably was stable at 3500°K in the primordial atmosphere, the stability of OH^{-1} in an oxide melt at 3000°K and above under tremendous pressure may be another matter. Because the solubility of water in any abundance in any melt depends on the formation and preservation of hydroxyl radicals, the solubility of water in an oxide melt depends more on the stability of OH^{-1} in such a liquid than on the partial pressure of water-gas in the atmosphere above the melt. In other words, no matter how high the partial pressure of water-gas in the atmosphere, the solubility of water in the melt will be minimal as long as the temperatures remain above those of hydroxyl ion stability. Unless OH^{-1} radical as such can be accepted in the oxide melt, in the equation (3) K and O^{-2} will be very large and OH^{-1} and H_2O quite small. The comparatively low melting points of the hydroxides of the common metals indicate

$$\frac{[H_2O]\ [O^{-2}]}{[OH^{-1}]^2} = K \qquad (3)$$

that the OH^{-1} is not stable at high temperatures. It seems likely, therefore, that the solubility of water in any primordial oxide melt would not have been great.

In the silicate melt (to which, in the cooling earth, the initial oxide melt appears to have been converted), on the other hand, Goranson's work indicates that water must have been soluble in considerable quantities, these being greater the higher the partial pressure of water-gas in the atmosphere (the confining pressure) and the lower the temperature. This statement must be qualified in its application by pointing out that the solubility of water in any silicate melt will drop off sharply above the upper temperature limit of the stability of OH^{-1} radicals in the melt. Once the melt temperature has dropped below that at which hydroxyl ions are stable in such melts, the equilibrium of equation (2) is driven to the right either by increasing the partial pressure of water-gas in the atmosphere or by decreasing temperature or by both acting together. In the primordial earth, the partial pressure of water-gas in the atmosphere must have decreased almost continuously with decreasing temperature because the fall of temperature would have permitted additional water to have dissolved in the melt and this water could have come only from the atmosphere. This decrease in the partial pressure of water-gas, however, would not have been, at any time, more than the amount that the temperature drop would have permitted to enter the melt, so the equilibrium of equation (2) would have been continuously displaced to the right, even though the confining pressure was steadily reduced.[6] This is particularly true when it is remembered that only the H_2O (as such) in the melt produces the melt's partial pressure of H_2O (gas). In other words, at any given temperature and pressure the value (concentration) of H_2O in equation (3) is the value to be put in the equation

$$\frac{H_2O \text{ (solution)}}{H_2O \text{ (gas)}} = K \qquad (4)$$

and not the total concentration of H_2O which has been accepted in the melt. It follows from this that the amount of

[6] It must be remembered that the confining pressure on the melt at any appreciable depth below the surface was furnished to a considerable extent by the overlying liquid. Thus, reductions in the partial pressure of water-gas in the primordial atmosphere would have affected only small decreases in the solubility of water in all but the very near surface portions of the earth-magma, and they would probably have been more than balanced by increases in water solubility due to falling temperature.

water ultimately taken up by the earth's silicate envelope could have been, and probably was, many times the amount of water left in the atmosphere after all the primary silicate magma had crystallized.[7] A relatively small and gradually decreasing amount of water in the atmosphere throughout the solidification of the earth-magma would, in other words, have been able to hold a much greater and increasing quantity in solution in the melt, largely as OH^{-1}.

Once the temperature of hydroxyl ion stability is reached, there is a direct relationship between the amount of water that can be dissolved in a silicate melt and the O^{-2} concentration of that melt. The effect of this relationship on the solidifying earth can be seen by examining the equilibrium of equation (3). From this equation it is apparent that the greater the concentration of O^{-2} in the portion of the primary earth-liquid in question, the less would have been the concentration of H_2O (as such) in that melt in relation to O^{-2}. However, the greater the total O^{-2} in the silicate liquid under any given conditions, the greater the total amount of H_2O that could be dissolved and held, mainly as OH^{-1}.

It also appears reasonable that the solution of water in the primary silicate melt could not have taken place in any large amount until the OH^{-1} ion was stable in that melt. The reaction of equation (2) in silicate melts, as in oxide melts (see pp. 519-520), may be shifted to the left at high temperatures sufficiently to prevent the formation of any considerable amount of OH^{-1} and, therefore, to prohibit the solution of much water in the early (higher temperature) stages of the primary earth-magma. To say this another way, the silicate

[7] The temperature of formation of water is high enough (as has been mentioned) that water would have been present abundantly in the atmosphere during almost all, if not all, of the time in which the molten silicate shell was developing and solidifying around the iron-nickel core. Through this period water would have been taken into in each increment of shell as it formed in amounts governed by the then prevailing partial pressure of water-gas in the primordial atmosphere and by the solubility of water as set by the chemical character of the silicates in that particular portion of the shell. As further increments were added, the previous ones would sooner or later have been solidified or buried deeply enough to prevent them from having been any longer in equilibrium with the partial pressure of water-gas in the atmosphere. These buried portions of the melt would not have lost what water they contained when they crystallized; it would have been combined in some of the minerals formed, either as OH^{-1} radical or water "of crystallization."

anions which will accept OH^{-1} ions apparently are not stable at high temperatures. With falling temperature, however, it may be inferred from Goranson's work that OH^{-1} and OH^{-1}-accepting silicates become more stable and the equilibrium of equation (2) shifts to the right. Applying this concept to the cooling primordial earth-magma would mean that its ability to dissolve water would become greater as the temperature dropped. It is probable, then, that the beginning of the acceptance of OH^{-1} in silicate anions bore a direct relation to the degree of water solubility in that melt (see equation 5) and until the temperature dropped sufficiently for

$$\underset{\text{(uncombined)}}{H_2O+O^{-2}=2OH^{-1}} \qquad \underset{\text{(in silicate anions)}}{=2OH^{-1}} \qquad (5)$$

double amphibole chains to become stable, water solubility in that silicate melt was low no matter what the partial pressure of H_2O-gas in the atmosphere. If this last explanation of the increasing solubility of water is the correct one, it follows that the solubility of water during the formation of the liquid earth would have been relatively small in the temperature ranges above those of double-chain silicate stability. Before the double-chain stage, the silicate anions formed, in a magma of the average composition of the silicate portion of the earth, almost certainly would have been olivine tetrahedra and single pyroxene chains. In such a melt, O^{-2} would have been available in considerable quantity to combine with H_2O to form OH^{-1} but the lack of acceptance of OH^{-1} ions in tetrahedra and single chains would have held the equilibria of equations (2) and (5) well to the left and prevented the solution of much water in the melt. Thus, only when the temperature of the molten earth was lowered enough to permit not merely the formation, in the melt, of fluid silicate anions in general but particularly the development of those alumino-silicate anions (double amphibole chains and mica sheets), in which OH^{-1} is accepted concomitantly with Al^{+3}, does it appear that water would have been soluble in appreciable amounts in the primordial magma.

The foregoing speculations on the reasons for the increase of the solubility of water in a cooling silicate melt (under a relatively constant confining pressure) in which double chains and sheet anions are present seem reasonable. Even if they

are not correct, however, the fact remains that water dissolves in silicate melts through combination with O^{-2} to form OH^{-1} and that this reaction is carried farther toward completion as temperature drops. Thus, it appears probable that the primary earth-magma, at least for some time prior to the beginning of crystallization, would have contained dissolved in it a considerable and increasing portion of the water available in the gaseous form in the gas-liquid mass of the earth. The remaining fraction would have been in the gaseous atmosphere and would, at final solidification, have been reduced in amount to that now in the ocean, less that which has been added from secondary magmatic sources in the earth as they solidified. The major question to be answered concerns the fate of that dissolved water as the primary magma crystallized but before it can be answered it is necessary to consider what happens to the water contained in a magma (as OH^{-1} or H_2O) when it crystallizes.

WATER IN A SLOWLY CRYSTALLIZING MAGMA

It is probable that the magmas, from which truly intrusive igneous rocks now exposed at or near the earth's surface were derived, solidified slowly because the high insulating quality of the thicknesses of unmelted rock by which they were surrounded prevented any but the slowest heat loss (Slichter, 1941, pp. 580-582). These magmas may have been produced from the melting (and intrusion) of: (1) igneous rock formed in the original crystallization of the primordial earth-magma, (2) igneous rock formed from the crystallization of post-primordial magmas developed after the original earth-solidification, (3) mixtures of derivative or secondary (post-primordial) igneous, metamorphic, or sedimentary rocks, or (4) metamorphic or sedimentary rocks. If the slow loss of heat were the only phenomenon to affect the cooling magma, its capacity to retain any water inherited from its primordial parent rock would be steadily enhanced. Concomitantly with cooling, however, two other changes are being carried out. The first of these is a loss of pressure which is most pronounced in magmas that have moved farthest upward and have shattered most the rock cover between themselves and the earth's surface. As the equilibrium of equation (2) is driven to the left by lowered pressure the quantity of actual H_2O

and O^{-2} in the magma will tend to increase and that of OH^{-1} to decrease. The second of these is the sequence in which crystallization takes place in the magma as temperature and, probably in most cases, pressure drop. In any known natural silicate magma, the bulk of the first minerals to form will be low or lacking in OH^{-1} radicals. In a basic magma, the early formed olivines, pyroxenes, and feldspars will be entirely wanting in OH^{-1}. Even in granite magmas whose earliest formed dark mineral is amphibole, the major portion of the minerals crystallized are anhydrous feldspars. The precipitation of these water-free minerals will result in an increase in the remaining melt, no matter what its composition, of OH^{-1} relative to O^{-2} and, therefore, an increase of H_2O at the expense of OH^{-1} to maintain the K of equation (3). At the same time, the precipitation of minerals high in oxygen in relation to silicon, so drastically reduces the O^{-2} content of the magma that the tendency of the remaining melt to maintain the K of equation (3) forces a marked break down of OH^{-1} to reinforce the diminishing concentration of O^{-2}. Both reduction in O^{-2} and increase in OH^{-1} concentrations act in the same direction, the production of O^{-2} and H_2O at the expense of OH^{-1}. Only if the tendency to change the equilibrium of equation (3) in the direction of more OH^{-1} and less O^{-2}, resulting from the fall of temperature, exactly balances or exceeds the tendency to change in the opposite direction produced by the crystallization of anhydrous minerals, will no OH^{-1} be broken up to form H_2O and O^{-2}.

There is, then, a considerable likelihood that any crystallizing magma, relatively rich in water to begin with, will become richer in water than it can hold in solution as OH^{-1}. Nevertheless, if the parent rock from which the magma came contained only a per cent or slightly more of water (as H_2O and OH^{-1}) it is highly questionable if any appreciable amount of that water would be separated during crystallization and driven out of the magma chamber.[8] With a fixed and small

[8] This is true whether the water-poor parent magma were completely or partially melted. If any normal igneous rock, not a product of the earth's initial crystallization, were partially melted, the resulting melt almost certainly would contain much of the water (as OH^{-1}) present in the original rock and the water content of the melt would be relatively high. This is so because the OH^{-1} radicals would have been largely in the minerals which would have melted first. Unless a magma produced by partial melting

quantity of water, it appears probable that the tendency in such magmas toward a decrease in the concentration of OH^{-1} (caused by the reduction of O^{-2} and the resulting breakdown of OH^{-1}) would be balanced or very nearly so by the tendency to increase OH^{-1} (caused by the increase in ability of the solution to hold OH^{-1} with falling temperature). In other words the decrease in the value of K in equation (3) as the magma cools probably is at such a rate as to be almost exactly that necessitated by the reduction of O^{-2} caused by the precipitation of anhydrous silicates, and little change in OH^{-1} or H_2O would result. This would mean that in a slowly cooling magma, derived from the complete or partial melting of such igneous rocks as are known at or near the earth's surface, the value of OH^{-1} would remain nearly constant and the magma would recrystallize to much the same mineral composition as its parent rock originally possessed. Only a very considerable drop in confining pressure would permit the loss of any important quantity of water from such a magma.

NORMAL IGNEOUS ROCKS AS A SOURCE OF ORE-FORMING FLUIDS

It has just been put forward that water in a slowly cooling and crystallizing magma will, if present in excess of a per cent or so, gradually increase in amount relative to the silicates

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were intruded near enough to the earth's surface to allow it to boil, it is probable that the water it contained (mainly as OH^{-1}) would be entirely used up in the formation of the same, or very nearly the same, minerals which were melted originally. In other words, a granitic magma produced by the partial melting of a non-primordial basalt-gabbro would be no more likely to produce a water-rich ore-forming fluid than would a magma formed from the complete melting of a secondary and anhydrous granite. On the other hand, the partial melting of a primordial igneous rock would result in a water-rich magma containing more water than would be accepted in the silicates stable in the slowly cooling secondary magma. Thus, as would be true of a magma produced by complete melting of primordial rock (see pp. 529-530), an ore-forming fluid well might develop, with its principal constituent the water unacceptable in the silicate lattices. This fluid probably would contain most of the ore-forming elements (not taken into the silicates) present in the primordial rock. It follows, then, that any secondary igneous rock would be low in ore-forming elements, most of them having been removed by the original ore fluid. If a magma were generated by complete or partial melting of a secondary rock and were to approach near enough to the surface to boil, the water escaping from it would, therefore, be low in ore-forming constituents. The minor quantities of the ore minerals deposited around the vents of certain volcanoes may indicate the small amount of the ore elements carried by water boiled from magma derived from secondary igneous rock by either complete or partial fusion.

which make up the bulk of the liquid. If the amount of water (as H_2O and OH^{-1}) rises faster than the increase in capacity (as temperature falls) of the liquid to hold it, water (probably as a gas under high pressure) will separate from, or become immiscible in, the silicate liquid in the later stages of the cycle of crystallization. It also has been indicated, however, that the melting and slow recrystallization of a normal igneous rock under a high confining pressure should not produce any appreciable excess of water over the solution's ability to hold it. Thus it seems probable that water-rich ore-forming fluids are not produced in any quantity from magmas derived from normal (low water content) igneous rocks. Of course, if such low-water magmas are intruded near, or extruded on, the earth's surface, they may lose water because of the great drop in confining pressure from that under which the igneous parent of the magma was crystallized. This tendency may on occasion be counterbalanced or overcome by the rapidity of crystallization, the viscosity of the melt (in the more silicic magmas), and the addition of water of meteoric origin, trapped but not truly dissolved in the melt. The near-surface final resting place of such magmas, however, makes it unlikely that, even when they give off water in quantity, they produce ore fluids capable of emplacing the usual sequence of ore deposits. It follows, therefore, that ore fluids are not derived from igneous magmas unless these magmas come from igneous rocks of a much higher water content than those known on or near the earth's surface. The possibility of the existence of such rocks is discussed in the next few paragraphs.

WATER IN THE CRYSTALLIZATION OF THE MOLTEN EARTH

In the crystallization of the primordial earth-magma, conditions probably were much different from those of a post earth-solidification magma. The molten globe was throughout its crystallization in contact with an atmosphere that exerted on it a far greater confining pressure than that of the present day. The presence in that atmosphere, during most of the earth's solidification, of considerably more water than the oceans now contain made possible the solution of water in at least the outer portion of the earth-magma in an amount not less than several times the weight of water remaining as a gas in the atmosphere (see pp. 521-522). The presence of so

relatively large a quantity of water in the primordial liquid after OH^{-1} became stable and before the melt crystallized, is no guarantee that the solid product would be any different in water content from normal igneous rock. Granted that the cooling of the molten globe was a slow process, following the early development of a solid crust, the process of crystallization of the magma beneath would have required so long a time as to permit equilibrium to have been maintained in the cooling mass. This would have resulted in the segregation, sooner or later, of an immiscible water-rich fluid phase, most of which would have penetrated the earth-crust and joined the atmosphere or the universal ocean, depending on the atmospheric temperature and pressure obtaining at the time the water escaped. If the large part of the water of the present ocean came, then, from the boiling of the primordial magma during earth solidification, the total amount of water present in the entire earth-mass during the crystallization of the globe would have been far less than if essentially all the water dissolved in the primordial magma had remained in the solid product and had been released later only in small part to the atmosphere and hydrosphere. Is it possible to determine which condition obtained, loss to the surface of most of the water dissolved in the original earth-liquid or the loss of little, if any, of it until after earth-solidification was complete? It has been put forward with what seem to be most convincing arguments (Slichter, 1941, pp. 582-583) that the earth-liquid cooled quite rapidly and solidified from the *inside* out. Slichter (1941, p. 567) has suggested less than 20,000 years for the time required to solidify that portion of the earth-magma from the core outward.

If this seemingly reasonable estimate of the time required for the solidification of the entire silicate shell is correct, the crystallization processes of the primordial magma well may have been quite different from those of a magma derived from the melting of normal igneous rock, enclosed on all sides during solidification by thousands of feet of solid rock. Instead of there being more time than needed for such adjustments of the composition of growing crystals to maintain equilibrium in the cooling earth, crystal development would have been much more rapid and haphazard than in a slowly solidifying earth. Instead of OH^{-1} ions being steadily broken

up as O^{-2} was removed from the melt, many, and perhaps most, of the OH^{-1} radicals present, at least in the outer part of the earth-magma, would have been accepted in silica tetrahedra (with Al^{+3} and similar ions substituting for Si^{+4} to maintain electron balance) and (connected by oxygen bridge ions) precipitated as amphibole double chains and mica sheets instead of the single olivine tetrahedra or pyroxene single chains which would have been formed had much more time been available for the continuing attainment of equilibrium.

If this concept of the crystallization of the primordial silicate shell around the molten core is correct, then the original igneous rocks of the earth would have been far richer in water-(hydroxyl-) bearing minerals and perhaps in water than those secondary ones now met in the field. Thus the possibility of the development of a water-rich ore fluid from a magma derived by the melting of a volume of primordial igneous rock seems to be good and merits a more full discussion in the next section.

CRYSTALLIZATION OF MAGMA FROM PRIMARY IGNEOUS ROCK

If a volume of primary igneous rock, produced by rapid crystallization of a water-rich primordial magma, is remelted, wholly or partly, the crystallization of the derivative magma will be a great many times slower than the original solidification. In this second and slow crystallization of a water-rich magma may lie the explanation for the development of a water-rich ore fluid as an end-stage product of that process. In this magma the rate of crystallization will permit the selection of the ions preferred, and the rejection of those not so acceptable, in the crystal lattices of the minerals being precipitated. This time for selection will permit the formation of a larger proportion of anhydrous, non-hydroxyl-bearing minerals and result in an increase in OH^{-1} concentration in the remaining liquid. The resulting decrease in O^{-2} and increase in OH^{-1} will be too great to be compensated for by the reduction of the K of equation (3) incident to falling temperature. This in turn will cause a breakdown of OH^{-1} to form O^{-2} and H_2O. Sooner or later, depending on the original water content, the water will be too large in amount to remain in solution (water cannot, because of the steady drain on O^{-2}, combine with O^{-2} to form OH^{-1}) and it will become

a dense gas phase gradually increasing in amount. This water-rich fluid will not, of course, be pure water since there will be a competition between the two media (water and silicate melt) for the constituents dissolved originally in the magma. The division of these between the two fluids will depend (so long as the two are in contact) on the relative solubility of the ions in the two media and their original concentration in the magma.

If immiscible sulfide melt is present in contact with the silicate melt, the development of the water-phase will profoundly effect the equilibrium relations and well may result in the extraction by the water-rich fluid (if sufficiently alkaline) of at least the more amphoteric of the sulfides, a concept which I hope to amplify later in a separate paper.

IMPLICATIONS OF WATER-RICH PRIMARY IGNEOUS ROCKS

If the primary earth-mass (or its outer shell of 50km or so at least) is markedly more water-rich than the secondary igneous rocks derived from a remelting and resolidification of such primary igneous material, it follows that magmas developed by remelting of primordial rocks are much more likely to produce ore-forming fluids than are those magmas that derive from secondary igneous rocks. This would indicate that the reason for the bulk of known igneous rocks failing to show any ore mineralization within their confines or beyond them is that such rocks probably solidified from secondary magmas and not from primordial rock remelted for the first time.

Because of the relatively low temperatures at which hydroxyl-bearing silicate anions become stable, it is unlikely that the bulk of the silicate shell surrounding the earth's core is water-rich. Only the outer portion of that mass should contain an abundance of water and the bottom of this water-rich zone well may be the base of the asthenosphere (Daly, 1943, p. 427). If this is correct, a magma (formed from primordial rock), which is generated or approaches within several miles of the earth's surface should usually be capable of producing a water-rich, end-stage fluid. This fluid probably would, when first formed, be above its critical temperature but would be contained under so great a pressure as to approximate the density of water and would be, therefore, nearly

as capable of dissolving other substances as liquid water. The ions so taken into the various solutions so produced well may include (in sufficient abundance) those which constitute the wide range of hydrothermal ore bodies.

CONCLUSIONS

It would appear reasonable that the outer shell of rocks of the primordial earth may have contained appreciably larger quantities of water than igneous rocks now subject to observation and analysis. This high water content probably was incorporated in these initial rocks by the combination of a high partial pressure of water in the primitive atmosphere and the rapidity with which the initial crystallization of the earth-mass was accomplished. If such water-rich rocks were remelted, the resulting magmas probably crystallized so much more slowly than the original fluid earth that the water concentrations would gradually have been built up to a point at which they would have begun to be immiscible (as a dense gas) in their parent melts. The reason for the very different behavior of water under the different rates of cooling lies in the inability of a rapidly developing silicate radial to be as selective in the ions with which it is being built as a similar radical would be in a slowly solidifying silicate fluid. Far more OH^{-1} would be accepted (paired with trivalent cations) in substitution for O^{-2} (plus Si^{+4}) in silicate anions formed in a quickly freezing silicate liquid than would be accepted in such a liquid crystallizing much more slowly. The immiscible, dense water-gas would usually be produced in slowing cooling melts derived from water-rich primordial rocks but rarely or not at all from melts developed from secondary igneous rocks. Such water-gases (shortly cooled and condensed to liquids in most instances) well may have been the ore-forming fluids of hydrothermal ore deposits.

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which merit a fuller treatment than I am able to give. I have, however, indicated them in footnotes in the body of the paper and have attempted to discuss them so far as I can.

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The Pennsylvania State College
State College, Pennsylvania

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A NOTE ON THE PLIO-PLEISTOCENE BOUNDARY IN THE SIWALIK SERIES OF INDIA AND IN JAVA

DIRK A. HOOIJER and EDWIN H. COLBERT

ABSTRACT. The definition of a boundary between the Pliocene and the Pleistocene in India has been a subject for much debate, as it has in other parts of Asia. The authors agree with the definition of the beginning of the Pleistocene period adopted at the 18th session of the International Geological Congress in 1948, where it was decided that "the lower Pleistocene should include as its basal member in the type area the...Villafranchian." In India the primitive elephant *Archidiskodon*, a characteristic Villafranchian genus, first appears in the Tatrot zone of the Siwalik series. However, certain other genera characteristic of the Villafranchian, such as *Equus* and *Leptobos*, have not been found below the Pinjor zone of the Siwalik series. When a complete survey of the Tatrot fauna is made it is found that there are nine holdovers from the distinctly Pliocene Dhok Pathan zone into the Tatrot, while six genera in the Tatrot may be considered as newcomers. Consequently the Tatrot appears to be truly transitional between the Pliocene Dhok Pathan and the Pleistocene Pinjor.

AT the 1948 session of the International Geological Congress in Great Britain[1] it was unanimously agreed that "the Lower Pleistocene should include as its basal member in the type area the Calabrian formation (marine) together with its terrestrial equivalent the Villafranchian."

The Villafranchian (Haug, 1908-1911, p. 1767) is defined by a widespread mammalian fauna characterized by the first appearance on several continents of *Archidiskodon*, *Equus*, and advanced bovines (*Leptobos*). Some of the implications of the definition recommended have been discussed by King and Oakley (1949, p. 186) who venture to state that the Pinjor stage of the Upper Siwaliks as well as the Djetis beds of Java are equivalent to the Villafranchian and hence basal Pleistocene. In other words, the Plio-Pleistocene boundary, according to their statement would have to be placed between the Tatrot zone and the Pinjor zone of the Siwalik series, and between the Kali Glagah beds and the Djetis beds in Java.

This is exactly what has been advocated by Pilgrim (1938, p. 479) and Von Koenigswald (1935; 1940, p. 77; 1950,

[1] International Geological Congress, Report of the Eighteenth Session, Great Britain 1948, Part I, General Proceedings, London, 1950, p. 214.

p. 93). In Java it has been the usage of the Geological Survey to regard the base of the Poetjangan beds as the base of the Pleistocene, and the Poetjangan beds contain the Djetis fauna of Von Koenigswald.

However, in the meantime a different view became prevalent through the writings of De Terra and Teilhard de Chardin (1936, p. 822), Lewis (1937, p. 192), Colbert (1940, p. 9; 1942, p. 1454; 1943, p. 426), Paterson (1941, p. 413), and Movius (1944, p. 84; 1949, pp. 346-347). All of these six authorities assign to the Tatrot zone a place just within the Pleistocene, and, with it, the Tjidjoelang plus Kali Glagah faunas of Java which have always been correlated with the Tatrot fauna of the Siwaliks.

Lewis (1937, p. 198) states that the genus *Archidiskodon* is represented at the type locality at Tatrot, thus establishing the fact that *Archidiskodon* appears earlier than *Equus* in the Siwalik system. Von Koenigswald's record of *Archidiskodon* from the Tjidjoelang plus Kali Glagah faunas of Java (Von Koenigswald, 1940, p. 74; 1950, p. 92) certainly is in favor of the view, so often expressed, that the Tjidjoelang and Kali Glagah faunas of Java should be regarded as Villafranchian, hence Pleistocene (Hooijer, 1950, p. 37). Von Koenigswald (1950, p. 93), however, states that he cannot accept this because the Tjidjoelang fauna "is associated with the marine Cheribonian fauna of Oostingh, which by all standards is of pliocene age."

Equus does not occur in Java except as introduced by Man. This genus makes its first appearance in the Pinjor zone as far as the Siwalik series is concerned[2].

Leptobos is found in the Siwaliks only in the Pinjor zone (Pilgrim, 1939, p. 7), while in Java this genus is represented in the Djetis fauna exclusively (Von Koenigswald, 1935, p. 7; 1940, p. 61) unless the skulls collected in Java by Dubois

[2] Lewis (1937, p. 198) writes that *Equus* first appears in the Tatrot formation, but Lewis's Tatrot formation is the Tatrot zone plus the Pinjor zone of Pilgrim and later writers, none of whom follows Lewis in uniting these two zone faunas. Although Pilgrim (1938, p. 446) had remarked upon the apparent absence of *Equus* from the Tatrot zone, Lewis's remark gave rise to some confusion; and subsequent to Lewis's paper *Equus* is stated to occur in the Tatrot zone by Von Koenigswald (1940, p. 74), Paterson (1941, p. 414), and Colbert (1943, p. 427). However, present evidence indicates that the first appearance of *Equus* is in the Pinjor zone.

(1908) do not belong to the Djetis fauna, a point that unfortunately cannot be definitely settled.

It has been stated by one of us (Colbert, 1943, p. 426) that the Villafranchian aspect of the Tatrot fauna is strong. As far as known at the time (Colbert, 1935, pp. 29-36; Pilgrim, 1939, pp. 5-7) only three Tatrot genera, *viz.*, *Ramapithecus*, *Dorcatherium*, and *Proamphibos*, failed to persist into the Pinjor[3], so that the Tatrot fauna seemed to be rather closely related to the succeeding Pinjor fauna which possesses *Archidiskodon*, *Equus*, *Leptobos*, etc.

The record of *Archidiskodon* from the Tatrot fauna by Lewis, (1937, p. 198) made us curious to see whether the Tatrot fauna might not be richer than was known thus far from the London, Calcutta, and New York Siwalik collections. We are much indebted to Dr. J. T. Gregory of the Peabody Museum of Yale University for his kind permission to restudy some of the fossil mammals collected by the Yale North India Expedition of 1931-1933. The examination of this collection proved to be very rewarding indeed: the list of mammal genera comprising the Tatrot fauna now appears to be more than twice as long as was known before, including *Archidiskodon* which, although recorded as from the Tatrot type locality already by Lewis (1937), apparently has been considered as absent from the Tatrot subsequently by Pilgrim (1938, p. 446; 1944, p. 32) and one of us (Colbert, 1942, p. 1448).

The complete list of Tatrot zone mammal genera as known at this moment is given in chart 1, in which the distribution of these genera in the Lower, Middle, and Upper Siwalik zones is also noted.

We have been able to add fourteen genera to the short list of Tatrot mammals. Not less than seven out of these newly added genera, *viz.*, *Paramachaerodus*, *Propotamochoerus*, *Hyosus*, *Hydaspitherium* (?), *Tragocerus*, *Selenoportax* and *Gazella* (?), do not pass from the Tatrot into the Pinjor, making a total of ten non-persistent Tatrot genera out of a total of twenty-four. Until now we knew of three non-persist-

[3] *Gazella* and *Hemibos* were mentioned as occurring with the Tatrot fauna by De Terra and Teilhard de Chardin (1936, p. 810), but for some reason or another, these genera have not been marked as from the Tatrot by Pilgrim (1939, pp. 5 and 7). *Gazella* would also have been a non-persistent Tatrot genus, which is difficult to believe since the genus is recent and still lives in India.

ent Tatrot genera out of ten (*viz.*, *Ramapithecus*, *Dorcatherium*, and *Proamphibos*), giving a percentage that does not differ significantly from the ten out of twenty-four, which we now have obtained.

On the other hand there are six genera that make their first Siwalik appearance in the Tatrot, *viz.*, *Ramapithecus*, *Pentalophodon*, *Archidiskodon*, *Sivachoerus*, *Potamochoerus*, and *Hemibos* c.q. *Bos*[4].

Thus, out of the twenty-four genera of mammals known from the Tatrot zone, nine are holdovers from the Dhok Pathan zone and do not persist into the Pinjor stage, while six genera are to be considered as newcomers. The remaining nine genera continue from the Dhok Pathan into the Pinjor.

CHART 1

Distribution of Tatrot zone genera through the Siwalik zones

	Lower Siwaliks		Middle Siwaliks		Upper Siwaliks		
	Kamlial zone	Chinji zone	Nagri zone	Dhok Pathan zone	Tatrot zone	Pinjor zone	Boulder Conglomerate zone
Ramapithecus					x		
Paramachaerodus				x	x		
Pentalophodon					x	x	
Stegodon				x	x	x	
Archidiskodon					x	x	
Hipparion	x	x	x	x	x	x	
Rhinoceros				x	x	x	
Tetraconodon	x	x	x	x	x	x	
Sivachoerus					x	x	
Propotamochoerus	x	x	x	x			
Potamochoerus					x	x	
Hyosus				x	x		
Hippohyus		x	x	x	x	x	
Dicoryphochoerus	x	x	x	x	x	x	
Sus		x	x	x	x	x	
Merycopotamus				x	x	x	
Hippopotamus				x	x	x	
Dorcatherium	x	x	x	x	x		
Hydaspitherium (?)				x	x		
Tragocerus				x	x		
Selenoportax			x	x	x		
Proamphibos				x	x		
Hemibos c.q. *Bos*					x	x	x
Gazella (?)		x		x	x		

[4] For the genus *Ramapithecus* this is its last appearance in the Siwalik series at the same time; *Ramapithecus* is not of great value in faunal comparisons, since the limits admitted to fossil Primate genera are very narrow.

One would perhaps be inclined to think that these figures indicate a more distinct break in the faunal succession between the Tatrot and the Pinjor than exists immediately below the Tatrot, and that consequently the Plio-Pleistocene boundary might better be drawn between the Tatrot and the Pinjor. However, the "invasion" of *Archidiskodon* had already taken place at least by Tatrot times[5]. Moreover, two of the "holdovers" from the Dhok Pathan, *viz.*, *Hydaspitherium* and *Gazella*, could not be identified by us with absolute certainty, and can better be left out of account here. Thus the ratio of the number of holdovers to that of the newcomers in the Tatrot zone fauna can be given as 7 to 6.

To the best of our present knowledge the Tatrot zone serves equally well, from the standpoint of the vertebrate paleontologist, as the beginning of a period as it would do as the end of one. We can only repeat what has already been said some years ago by one of us (Colbert, 1942, p. 1446), that the Tatrot is possibly transitional between the Upper Pliocene and the Lower Pleistocene. The same holds, mutatis mutandis, for the Tjidjoelang and Kali Glagah faunas of Java. No grounds are apparently present to consider these latter faunas, which contain *Archidiskodon* (Von Koenigswald, 1950, p. 92), as Plaisancian and Astian respectively (Von Koenigswald, 1950, p. 94), which would implicate a set of proboscideans far more primitive than those of which these Javan faunas give evidence.

[5] As very correctly remarked by Hopwood (1938, p. 473), it is difficult to apply the principle of simultaneously immigrating forms when dealing with areas at or near the (supposed) center of evolution of a given group.

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Rijksmuseum van Natuurlijke Historie
Leiden, Netherlands

The American Museum of Natural History
and Columbia University
New York, N. Y.

ESSAY REVIEW

LA TECTONIQUE D'ECOULEMENT PAR GRAVITE; GRAVITY GLIDING TECTONICS*

During the past decade or a little more, a set of ideas called collectively "la tectonique d'écoulement" has become prominent in Alpine, especially West Alpine, geology. According to these ideas, the far travel of some of the Alpine nappes or thrust sheets, for example the famous Préalpes, need not be explained entirely by shortening under tangential pressure but can be ascribed, at least in part, to gravitational sliding of the forward parts of the nappes down slopes from structural highs that were at the same time topographic highs. These ideas are not wholly new, as indeed their proponents clearly state, but the application of them has been more broad and thorough than before, and thus it constitutes a new departure.

The repercussions of these ideas have been reaching North American geologists lately, especially by the medium of personal contacts established at the International Geological Congress in Great Britain in 1948. The Editors of this JOURNAL had indeed arranged for Professor Gagnebin of the University of Lausanne, one of the principal proponents of the new views, to prepare a general article on the subject for American readers, but unhappily his untimely death intervened. The present note is designed to call attention to an extremely interesting and well balanced symposium on gravity gliding tectonics, arranged by Dr. L. U. de Sitter, Professor of Geology at Leiden, and published in *Geologie en Mijnbouw*, the organ of the Netherlands Geological and Mining Engineering Society, for December 1950 (Jaargang 12, nieuwe serie, no. 12, pp. 329-365). From this symposium, structural geologists not conversant with recent work in the Alps can obtain a very satisfactory general understanding of the present status of the matter. Five of the six papers in the symposium are in French, but van Bemmelen's is in English.

In a short introduction to the symposium, de Sitter points out that the proponents of gravity gliding tectonics, like the proponents of most new sets of ideas, include both moderates and extremists, and he has neatly balanced the two points of view in the invited papers. The moderate position is set forth in a long article by Tercier on the Swiss Alps (containing the only adequate bibliography in the symposium) and a shorter one by de Sitter on a part of the Italian Alps (a sort of supplement to his report on the same area, reviewed in this JOURNAL, September 1950, vol. 248, pp. 669-670). According to them, gravitational sliding may help greatly

*The primary meaning of the verb écouler is flow, but in the present context it corresponds rather to the English glide or the German gleiten.

in explaining many puzzling features of Alpine geology, but it is essentially a minor surface phenomenon on the flanks of topographic and structural highs, which, like the other major features of Alpine structure, were produced in the "classical" maner by shortening under tangential pressure. This was essentially the view of Gagnebin also, though with regard to specific cases he was considerably less conservative than Tercier. The more extreme position is represented on the other hand by Gignoux, who deals mainly with the French Alps, and by van Bemmelen, who deals mainly with Indonesia; each of them here summarizes a recent more complete statement of his views (for van Bemmelen's, see The Geology of Indonesia, reviewed in this JOURNAL, June, 1951, pp. 462-466). These men generalize the theory of écoulement to the view that gliding and flowage are not surficial phenomena only but may affect a good part or the whole of the thicness of the Earth's "crust." Gignoux in particular now thinks less in terms of the first-broached surficial gliding tectonics than in terms of deep-seated "flow tectonics" (to use a distinction suggested by de Sitter). Moreover, he subordinates tangential forces as prime movers to vertical forces, in an essentially viscous medium (suction, intumescence); van Bemmelen entirely rejects tangential forces and explains all tangential movements by gravity alone. These views are of course descended from those of Haarmann (Die Oszillations Theorie, 1930), as both Gignoux and van Bemmelen state, but they have been greatly improved and strengthened by the younger men in the light of their extensive field experience. Finally, in a subtle theoretical analysis, Goguel examines certain implications of the theory of plastic and viscous matter (as differentiated by the presence or absence of a finite elastic limit) and shows that, as far as present model theory extends, the facts of observation support the view that the rocks of the "crust" are plastic and have a real fundamental strength.

The implications of these new views for American geology are not far to seek. Already in the early '30s, Bucher reached a somewhat similar explanation for the Heart Mountain and McCullough Peaks klippen of the Heart Mountain thrust in northwestern Wyoming. The ideas may also be applicable to parts of other spectacularly far-travelled thrust sheets in the Cordillera, for example some of those in the southern Great Basin, or in the Appalachians, for example the Taconic klippe of Vermont and New York, though that has apparently undergone an additional deformation since emplacement, obscuring possible evidence. Geologists who wish to learn about and assimilate this new conceptual tool in structural geology will find de Sitter's symposium indispensable.

JOHN RODGERS

REVIEWS

Response of Physical Systems; by J. D. TRIMMER. Pp. ix, 268. New York, 1950 (John Wiley & Sons, Inc., $5.00).— This new work represents a generalized approach to the fields of instrumentation and measurement. The author undertakes a short semi-philosophical introduction which might be taken as vague, but which does not detract from the positive merits of the book.

Such positive merits do exist; for example the use of a unified and consistent notation in the entire treatment of systems of linear differential equations as applied to the physical problems in the book. The use of the concepts of error and delay is clear and consistent throughout, as well.

A comparatively large range of topics is covered, most of the specific examples given evidently arising from the author's industrial background. However, this fact does not in any sense classify the text as a handbook. Consider how briefly a two hundred and sixty page work must sketch such topics as: Physical Systems, First-Order Systems, Second Order Systems, Sinusoidal Forcing of Linear Systems, Higher-Order Systems, Measuring Instruments, Feedback Systems, Parametric Forcing, Distributed Systems, Nonlinear Systems, and the Laplace Transform in the Study of Linear Systems. I agree with Professor Trimmer in his choice of differential equations for expository convenience. There is a satisfactory appraisal and presentation of the working procedures of the Laplace Transform, so that the work should be complete in this respect.

Mr. Trimmer does Trojan service by presenting in one work the relations and methods which are used, but vaguely comprehended or correlated by most practical workers in engineering and physics. The only general criticism which may be laid against Response of Physical Systems is the occasional transition it makes to rather sophisticated concepts and methods, without going into adequate detail. Generally, however, "Response" is the only book of its sort at present, and fulfills the need for a survey of instrumentation which applied physicists and engineers require. It is hoped that the present work presages a flourishing development of literature which clearly presents the problems and solution of problems in design.

NOEL STONE

Time and Its Mysteries; by H. N. RUSSELL, A. KNOPF, J. T. SHOTWELL, and G. P. LUCKEY. Pp. x, 126. New York, 1949 (New York University Press, $3.00).—No concepts hold greater fascination for the reflecting mind than space and time, these well-springs of problems and paradoxes. Through them man gained his first glimpse of infinity, and yet he finds all his experience within them and bound by them. The most interesting developments in science

at every stage are somehow related to changes in the ideas of time and space. To sponsor periodic lectures on "Time and Its Mysteries," as the James Arthur Foundation does, is therefore to present a significant pageant of discovery which is instructive and useful in many ways.

The book under review is the third in this distinguished series and comprises four well-written lectures by authorities in their respective fields. The first, by Henry Norris Russell, is entitled "The Time Scale of the Universe" and sets forth the astronomical evidence which leads us to believe the universe to be 1840 million years old. It concludes with a brilliant and illuminating account of the life cycle of a star as it is conceived in modern physics.

Adolph Knopf presents the geologist's counterpart of the astronomical evidence. He carefully develops and then sifts with expert skill the data which form the basis for the geologist's judgment of the age of the earth, 2000 million years or more. I have often been puzzled by the claims made by men working in the theory of relativity, claims concerning a disagreement between the astronomical and the geological ages of the universe. The chapters by Russell and Knopf state precisely what is here involved, and their juxtaposition in this book is a most felicitous circumstance.

James T. Shotwell reports on "Time and Historical Perspective," embedding time into the theme of history. Full of interesting facts, this third lecture surveys the means employed for dating events by historians of all ages and shows how the contact between astronomy and history was finally made. The lecture was given too early (1946) for an inclusion of the interesting C^{14} method.

The last chapter is very appropriately devoted to a more practical topic, the "Development of Portable Time Pieces." George P. Luckey, of the Hamilton Watch Company, is its author. He succeeds well in giving the reader an insight into the history of time keeping and into many of the clockmaker's secrets.

HENRY MARGENAU

Fundamentals of Optics, 2d ed.; by FRANCIS A. JENKINS and HARVEY E. WHITE. Pp. xi, 647. New York, 1950 (McGraw-Hill Book Company, $7.00).—This is an extensive revision of the popular "Fundamentals of Physical Optics" published in 1937. The change in title was made necessary by the addition of ten chapters devoted to geometrical optics and a final chapter on quantum optics. The extension into geometrical optics will be welcomed by many instructors who wish to include at least a brief review of this subject in the single course in optics permitted in the crowded course schedules of advanced undergradute physics majors, and the inclusion of a chapter on the quantum behavior of light is certainly

desirable even though photons are discussed at length in all books on atomic physics.

The frequent insertion of descriptions of experimental illustrations into the physical optics sections is a helpful device. Interference and diffraction phenomena are particularly well treated. New developments such as the phase-contrast microscope, the interference filter, the Schmidt camera, and the electron microscope are described, and there are extensive revisions of the treatment of some of the traditional topics in physical optics. The insertion of many new problems at the ends of chapters increases the usefulness of this volume as a text. Descriptions of laboratory experiments are not included.

Fundamentals of Optics constitutes, in the opinion of this reviewer, the most satisfying discussion at an intermediate level of the whole field of optics available at the present time. Because of the inclusion of geometrical optics this edition will undoubtedly be even more widely used than was the first. W. W. WATSON

Atomic Physics; by WOLFGANG FINKELNBURG. Pp. x, 498; 226 figs. New York, 1950 (McGraw-Hill Book Company, $6.50).—As the author states in the introduction, this book presents atomic physics in a broad sense. It deals with the structure, properties, and behavior of matter. No attempt is made to present all of the material in this vast field, which would be impossible in a single volume; instead the author's intention is to lead the reader to an understanding of the new concepts which are such an important part of "modern physics". He is singularly successful.

Atomic Physics begins with a discussion of the physical facts known about atoms. An examination of atomic spectra leads to the old quantum theory, thence to wave mechanics. The remainder of the book discusses nuclear physics, molecular physics, and finally the atomic physics of the liquid and solid states. The bibliographies with each chapter are sufficient, if not exhaustive.

Atomic Physics is written with the college senior and graduate student in mind. The meaning of the results is stressed, rather than the details. The problem of rigor is well handled. Either a satisfactory rigor is maintained, as in the quantum mechanics section, or the material is qualitative. There is no attempt at compromise. The book is equally well suited for use as a textbook for an introductory course in "modern physics" or for individual study. *Atomic Physics* is so written that even the expert in one of the particular fields discussed will gain from the opportunity to see his own work in relation to the rest of modern physics. Lastly, by his continued references to current research, and the philosophical aspects of the

work which has been done, Professor Finkelnburg has drawn a vivid picture of the present state and direction of development of physics.

D. E. HARRISON, JR.

Mathematics of Relativity; by G. Y. RAINICH. Pp. vii, 173. New York, 1950 (John Wiley & Sons, Inc., $3.50).—*Mathematics of Relativity* consists of a slow step-by-step generalization of plane analytic geometry into the n-dimensional space-time geometry in generalized coordinates. The tensor concept is introduced in the first chapter on "old physics." The second chapter builds the new geometry containing one imaginary coordinate which is required in the later work, and treats tensor analysis. The third chapter shows that physics does not quite fit this generalization, but that the required changes are those obtained by special relativity. All of the fundamental quantities are combined into what the author calls a "complete tensor." The fourth chapter develops the theory of curved spaces and leads to the introduction of a curvature tensor which is identified with the physical "complete tensor." The theory of tensor analysis is refined to apply to curved spaces. Chapter five shows how the purely geometrical results of chapter four may be identified with physical quantities, and gives a detailed discussion of three special cases.

This book is perhaps one of the most discursive mathematics texts ever written, but this statement is not meant as a criticism. The descriptive material is a great aid to the student, because the author has attempted to give the reader some intuition about what the mathematical concepts actually mean in terms of physically realizable quantities.

The professional mathematician may feel that the work is not sufficiently rigorous, but it will doubtless satisfy the physicist and the applied mathematician for whom it is written.

Mathematics of Relativity is an excellent introduction to the field, and as such is a valuable addition to the literature of relativity.

D. E. HARRISON, JR.

Electromagnetic Fields, Theory and Applications. Volume I, Mapping of Fields; by ERNST WEBER. Pp. xiv, 590; 173 figs. New York, 1950 (John Wiley & Sons, Inc., $10.00).—Static electric and magnetic fields and the methods of solving the family of potential equations are the subject of this book; a forthcoming volume will deal with the dynamic interaction of electric and magnetic fields and the methods of solving the family of wave equations. As the title indicates, the present volume is a comprehensive treatment of the several analytical and practical methods of field plotting. The reader is constantly advised whether the one method might be

more feasible or accurate than the other. The description of the practical methods, which takes up nearly one fifth of the book, includes graphical and numerical techniques, but also such experimental analogies as the electrolytic trough. The analytical methods described include the use of 'conjugate' functions in the solution of two-dimensional problems (featuring a very complete coverage of conformal mapping), and an extensive mathematical treatment of three-dimensional problems; this chapter is supported by a well-illustrated discussion of the several orthogonal coordinate systems employed. The close analogy between static electromagnetic fields and the fields occurring in fluid dynamics, heat conduction, and gravitational theory is emphasized and corresponding quantities are tabulated. Rationalized MKS units are used throughout.

Dr. Weber, who is Head of the Department of Electrical Engineering at the Polytechnic Institute of Brooklyn, has drawn on his obviously vast teaching experience to write an excellent textbook; by a special bibliographical system which refers to particular pages or chapters in other works, he has contrived to produce a very good reference book as well. The usefulness of this volume as a handbook of field solutions will probably prove to be the deciding factor in determining its popularity.

CHARLES SUSSKIND

Mineralogische Tabellen, 2d ed.; by HUGO STRUNZ. Pp. xiv, 308; 73 figs. Leipzig, 1949. (Akademische Verlagsgesellschaft, Geest and Portig; DM 21.00 bound; paper, DM 19.80.)—The first edition of this book appeared in 1941, and in 1944 it was reprinted in this country. The new edition contains several welcome features. An expanded Part I (Introduction to crystal chemistry) explains the classification of minerals, emphasizing the different aspects of crystal chemistry involved. Various kinds of chemical formulas are discussed. The main types of inorganic crystal structures are described and figured. According to the foreword, the systematic section of the book (Part II, pp. 65-236) has been brought up to date with new information on more than 300 minerals; in addition, descriptions of about 50 new species have been added. The total number of recognized minerals is 1124, of which more than half crystallize in the monoclinic or the orthorhombic system, a third in the hexagonal or the isometric system, and the remainder in the tetragonal or the triclinic system. Statistics of crystal classes, space lattices, and space groups might constitute an interesting and appropriate addition when another revision of this useful reference book is being prepared.

Part III (pp. 237-308) contains the index and list of synonyms

(about 5500 entries), and a short list of important recent texts and reference books on mineralogy and crystallography. Many of the synonyms are foreign names or spellings, permitting the location of most minerals by their common names in almost any language. The usefulness of this book must be emphasized many times over, both as a practical crystal-chemical classification of minerals and as a convenient reference in which to find certain crystallographic constants (contents and size of unit cell, and space group) of minerals.

HORACE WINCHELL

Races . . . A Study of the Problems of Race Formation in Man; by C. S. Coon, S. M. Garn, and J. B. Birdsell. Pp. xiv, 153; 15 plates, 11 figs. Springfield, Illinois, 1950 (Charles C. Thomas, $3.00).—The most effective teaching, according to these authors, is to present the best scheme possible from the evidence as a whole and treat it critically, rather than to plead ignorance or to speculate wildly. This volume, the first of a series of *American Lectures in Physical Anthropology,* is on racial variation in man, a subject which has left many scholars pleading ignorance. Whether the present authors have achieved their ideal of critical evaluation of the best evidence available depends largely on what distinction the reader is willing to make between provocation and speculation.

Three principal factors of racial variation discussed in the book are genes, the environment, and evolutionary status. The potentialities and complications of studying the genotypes in man are discussed in some detail. Although the mutation rate as known for man would but rarely account for visible physical characters, over time it must have been effective because: "The 'goodness of fit' between racial varieties of the human organism and the varieties of environment, cultural and geographical, in which the organism lives, is too nearly perfect." Many potential effects of the environment broadly defined, are suggested, particularly where the environments are extreme. Larger chests are an adaptive change to high altitudes where more oxygen is needed. Temperatures affect energy. The peoples of Southeast Asia, southern China, Sumatra to Japan are all reduced in size and the major feature shared by all is a rice diet. The tall Indians of our western plains were, on the other hand, meat eaters. The tall and thin, having the greatest body surface, are best adapted to extreme hot and dry areas, the short and fat to extreme cold. Some racial groups exhibit, in comparison with others, more of the known characteristics that were commoner at earlier periods in man's history. Some of the ones discussed are skull thickness, size of teeth, supraorbital ridges, and brain capacity.

The authors present a sixfold racial stock classification but insist that it is merely a convenience. On the other hand more space is devoted to a functional classification of races based on factors outlined above, such as evolutionary status, body build, and special surface features. Thirty races result.

As a teacher, the reviewer finds the book suggestive and certainly stimulating for further research. WENDELL C. BENNETT

Early Man in the New World; by KENNETH MACGOWAN. Pp. xv, 260; 85 figs., New York, 1950 (The Macmillan Company, $5.00).—As a history of the search for early man in the New World—what the searchers have done and how they have theorized—this is a useful book. The author is not a professional anthropologist (he was formerly a producer on Broadway and in Hollywood and is now Chairman of the Theater Arts Department at U.C.L.A.), but he has a broad knowledge of the subject and presents it interestingly. The coverage is good, and there is an excellent series of illustrations, which compensate for a lack of technical details. Chapters on Pleistocene geology and early man in the Old World provide more perspective than is usual in works of this kind.

The book is less successful as a record of what the search for early man has accomplished. The author has not attempted a synthesis of the results, nor to reconcile inconsistencies of viewpoint. By giving as much weight to extremist theories as to areas in which there is substantial agreement among specialists, he creates an impression of greater confusion than actually exists. For those who wish a balanced picture of the subject, therefore, supplemental reading of a more technical nature is recommended (e.g., Wormington's *Ancient Man in North America,* reviewed in the last volume of this JOURNAL, pp. 518-19). IRVING ROUSE

Symphony of the Earth; by J. H. F. UMBGROVE. Pp. xii, 220; 123 figs., 10 plates, index. The Hague, 1950 (Martinus Nijhoff, 8vo., paper cover, 11 guilders—about $2.86).—This volume, well printed and embellished with numerous attractive illustrations, attempts to explain earth science "in more or less conventional prose," with technical terms reduced to a minimum. Chapter I, entitled *Symphonia Terrestris,* is in effect a brief edition of the entire work, or, in musical parlance, a prelude setting the theme that is developed in greater detail by what follows. This theme has to do with movements of the earth's crust, especially mountain making, through the ages, and with the long history of organic evolution.

Chapter II, *A Country Below Sealevel,* outlines briefly the devel-

opment, by long-continued subsidence and sedimentation, of the great lowland that includes Holland. This is an example of large-scale basining, a common type of crustal deformation. Chapter III, *Across the Swiss Alps*, conducts the readers from the Jura folds southward across the Molasse trough and the successive zones of complex nappe structure in Switzerland and northern Italy. The author regards the deformation generally as the result of compression in the crust, but favors gravitational sliding as the best explanation of nappes in the Pre-Alps and in Lombardy. Crumpling of the thin sedimentary cover in the Jura, far north of the Alpine front, is ascribed to strut action of wedges in the old basement rock, and not to stresses transmitted through the thick sedimentary fill beneath the Swiss Plain.

Following a brief chapter that describes the deep troughs and intervening ridges in the Indonesian region, Chapter V discusses evidence for the concept that a downward bulge or "root" of continental rocks underlies the Alps. The hypothesis of Vening Meinesz, that the belt of strongly negative gravity anomalies in the East Indies reflects downward buckling of the crust, is indorsed for orogenic zones generally. *A Trip on a Volcano*, the final chapter dealing with physical geology, gives valuable information on Vesuvius and other volcanoes, considers the mechanism of volcanic and related intrusive action, and touches on the controversy regarding the origin of granite, with conclusions favorable to both camps—transformists and magmatists.

Life and its Evolution is much the longest chapter in the book. It cites and analyzes some evidence and ideas bearing on the mode and rate of evolution, and goes on to discuss attempts to interpret life by the three schools of thought known as vitalism, materialism, and holism. The fundamental distinction between living and non-living systems is of course the great mystery. Umbgrove favors Bohr's concept that processes governing the two kinds of systems are to be regarded as "complementary." Though this may be comforting from a philosophic point of view, it does not appear to solve the basic problem.

Dr. Umbgrove has read widely in several languages, and therefore his synthetic treatment is built on a broad base. His chapters are well written, and make interesting reading for anyone who has some knowledge of geology and paleontology. Although the effort to avoid technical terms is on the whole remarkably successful, the author realizes that parts of the subject matter will be difficult for some intelligent readers. Certain generalizations, especially those on periodicity of major geologic events, will be greeted with some head shaking. For example the curve for "mountain building" (figs. 8 and 18) represents two intervals of about 250 million years

each that look convincing to the uninitiated. Needless to say, construction of such a curve is conditioned by the author's conviction as to which orogenic episodes are of first rank; and Umbgrove's uniform spacing is obtained by representing a major episode exactly at the close of Precambrian time—a conclusion not justified by evidence now available. The curves for "Differentiation of Land Plants" and "Formation of Basins" also will be taken—in a favorite phrase of the author—*cum grano salis*. However, differences of viewpoint lend zest to scientific study. The book has much value. Numerous excellent block diagrams merit special commendation.

CHESTER R. LONGWELL

A Survey of Weathering Processes and Products, revised ed.; by PARRY REICHE. Pp. 95; 5 figs. Albuquerque, New Mexico, 1951 (University of New Mexico Press, $1.00, paper cover).—This bulletin, first published in 1945, was received so favorably that the edition was soon exhausted. The text has been somewhat changed and amplified, and the bibliography is increased from 67 to 88 titles. One of the added reference works is A. La Croix's "Les Laterites de la Guinée," in which exception is taken to Vageler's suggestion that scarcity of humus in the tropics is the controlling factor in laterization. La Croix was convinced that this viewpoint confuses cause and effect; and Reiche apparently has decided that the matter is at least in serious doubt, since the new edition omits paragraphs explaining the absence of humus accumulation in the tropics, together with the old figure 6, a graph showing the relation between bacterial optima and annual temperatures. This perhaps is the most conspicuous change made in revising the text.

Like its predecessor, the new bulletin gives an informative review of rock weathering and soil formation. Physical processes are outlined in the first seven pages; chemical processes and resulting products receive major attention. A number of the chief points concerning relative mobilities of rock constituents, and the "weatherabilities" of rocks, are expressed graphically in several diagrams. About 30 pages are devoted to soil-forming processes and soils, with clear explanations of podsolization, calcification, and typical profiles developed under contrasting climates.

The treatise was first prepared to be used as a text by students in a sedimentology course. The new edition should serve admirably for that purpose, and in addition it is a useful reference volume for the working library of every physical geologist.

CHESTER R. LONGWELL

PUBLICATIONS RECENTLY RECEIVED

Illinois State Geological Survey publications as follows: Bulletin 73, Bedrock Topography of Illinois, by Leland Horberg; Bulletin 74, Pennsylvania Spores of Illinois and Their Use in Correlation, by R. M. Kosanke; Report of Investigations 147, Illinois Mineral Industry in 1948, by W. H. Voskuil; Circular 164, Structures of Herrin (No. 6) Coal Bed in Marion and Fayette Counties, by Raymond Siever, Urbana, 1950.

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ERRATA

In the paper entitled "Allanite from Wilmot Pass, Fiordland, New Zealand," volume 249, 1951, the following corrections should be noted:

Page 211, second line below Table 2; page 212, second paragraph, line 2; last paragraph, line 3: For 0.01, read 0.001.

Page 212, second line from bottom: For 14-15, read 23.

I am indebted to Dr. J. P. Marble for calling my attention to these inaccuracies.

C. OSBORNE HUTTON

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